

ABSTRACT

Title of Dissertation: A DISCOURSE ON CHILD
MALNUTRITION: ANTHROPOMETRY,
EMERGENT THEMES, QUALITY
CONTROL MAXIMS, AND CLIMATIC AND
ECONOMIC DETERMINANTS

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Geographical Sciences

Malnutrition is a detrimental and significant plight for young children, responsible for 45% of all deaths among children worldwide. The aim of my dissertation is to assess the history of the science of anthropometry, synthesize the cumulative findings within the contemporary child malnutrition literature, dispute certain quality control maxims of anthropometric child-health surveys, and quantify the responsible latent factors of child malnutrition. These efforts are in service of a better characterization of malnutrition, a more reliable estimate of how many children are malnourished, and a better understanding of the geographical distribution and dynamic stochastic characteristics of malnutrition. It is essential to better understand malnutrition and its causes to suggest appropriate corrective policy. This dissertation consists of four principal essays, each from a unique conceptual perspective. The first essay is a historical and epistemological perspective of the science of anthropometry. I

contextualize the legacy of child malnutrition efforts, including the link between eugenics and contemporary notions of “normal” child growth, the institutional power-struggle for child growth chart superiority, the obfuscated distinction between growth references and standards of growth, and the consequences of universal standards that do not reflect observable populations. The second essay is a systematic review of the literature, the largest of its kind to date. I synthesize 184 disaggregate empirical studies of the determinants of child malnutrition in Africa published since 1990. I find numerous opportunities for development within this corpus, in particular opportunities to enrich the scope, scale, and quantification of the field. The third essay is an analytical perspective on the quality control mechanisms applied to anthropometric surveys. I challenge the practice of rejecting datasets based on overlarge z-score standard deviation values and offer an alternative approach. The fourth essay is an econometric empirical analysis in Kenya and Nigeria of child malnutrition determinants. I use spatial Bayesian kriging and four-level random intercept hierarchical logit models to show the spatial heterogeneity of malnutrition prevalence, and to quantify various socio-economic and climatic determinants of child malnutrition. I find significant spatial and hierarchical relationships and determinants, which can move malnutrition rates by over 50%.

A DISCOURSE ON CHILD MALNUTRITION:
ANTHROPOMETRY, EMERGENT THEMES, QUALITY CONTROL
MAXIMS, AND CLIMATIC AND ECONOMIC DETERMINANTS

by

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1 Introduction

1.1 Overview

This dissertation aims to expand the frontier of the science of anthropometry and child malnutrition. It adds to the current debate and cumulative extant findings within the literature, and analyzes the magnitude and geographical distribution of malnutrition to quantify the responsible latent factors. The dissertation consists of a collection of four standalone essays, which weave together a gestalt discourse. Topics range from the history of anthropometry, and a critical systematic literature review, to the debate of statistical quality control maxims, and an empirical analysis of climatic and economic determinants of child malnutrition.

1.1 Background Motivation

The specter of food supply falling behind population growth has long faltered the discussion of poverty and famines, most notably by Thomas Malthus (1798) in his infamous volume *An Essay on the Principle of Population*. Malthus concludes, “the power of population is indefinitely greater than the power in the earth to produce subsistence for man” (1798, p. 4). Malthus believed, “Population, when unchecked, increases in a geometrical ratio. Subsistence increases only in an arithmetical ratio” (1798, p. 4). To combat such mathematical certitudes, he proposed selective breeding among the wealthy and forced celibacy and sterilization of the poor, what Francis Galton (1883b) and his followers would later dub *eugenics*.

Fortunately, Malthus was wrong about the immensity of humanity’s ability to

better itself. Malthus patently failed to account for economic growth, innovation, dignity and liberty, which would result in a one-hundred-fold enrichment per person over the next two centuries (McCloskey, 2006, 2010). However, the very real and persistent obstacles of poverty and hunger remained.

Nobel Prize-winning economist and philosopher Amartya Sen (1976) conceived the paradigm of entitlements. Instead of asking is there enough food? Sen asked who is entitled to access food? (Devereux, 2018). He defined entitlements as “the set of alternative commodity bundles that a person can command in a society using the totality of rights and opportunities that he or she faces” (1984, p. 497).

Indeed, Sen noted, “Much about poverty is obvious enough. One does not need elaborate criteria, cunning measurement, or probing analysis, to recognize raw poverty and to understand its antecedents. It would be natural to be impatient with long-winded academic studies” (1981, p. vii). But Sen showed that not everything about poverty and hunger is quite so simple, especially when moving away from its most extreme and raw forms.

To diagnosis poverty and hunger in all its forms, Sen illuminated the economic mechanisms of ownership patterns, exchange entitlements, modes of production, and economic class structures. Furthermore, by recognizing the importance of wellbeing beyond income, Sen (1999) argued that health is of fundamental importance to economic development: transforming the study of poverty. Contemporary frameworks of malnutrition—including this study—derive much of their thrust from Sen’s insights.

1.2 Theoretical Framework

In this dissertation, I employ a holistic framework approach to motivate model specifications, etiological pathways, and causal inferences (Figure 1). This framework ties together anthropometric science, health surveys, and early warning systems, with hierarchical and causal structures that bridge conceptual and observable determinants of child malnutrition. Chapter 2 synthesizes the necessary foundational theories that are responsible for the framework. Chapter 3 surveys the application and findings of the framework in the literature. Chapter 4 critiques certain methodological practices. Chapter 5 contributes to the qualification and quantification of specific determinants and spatial hierarchical effects.

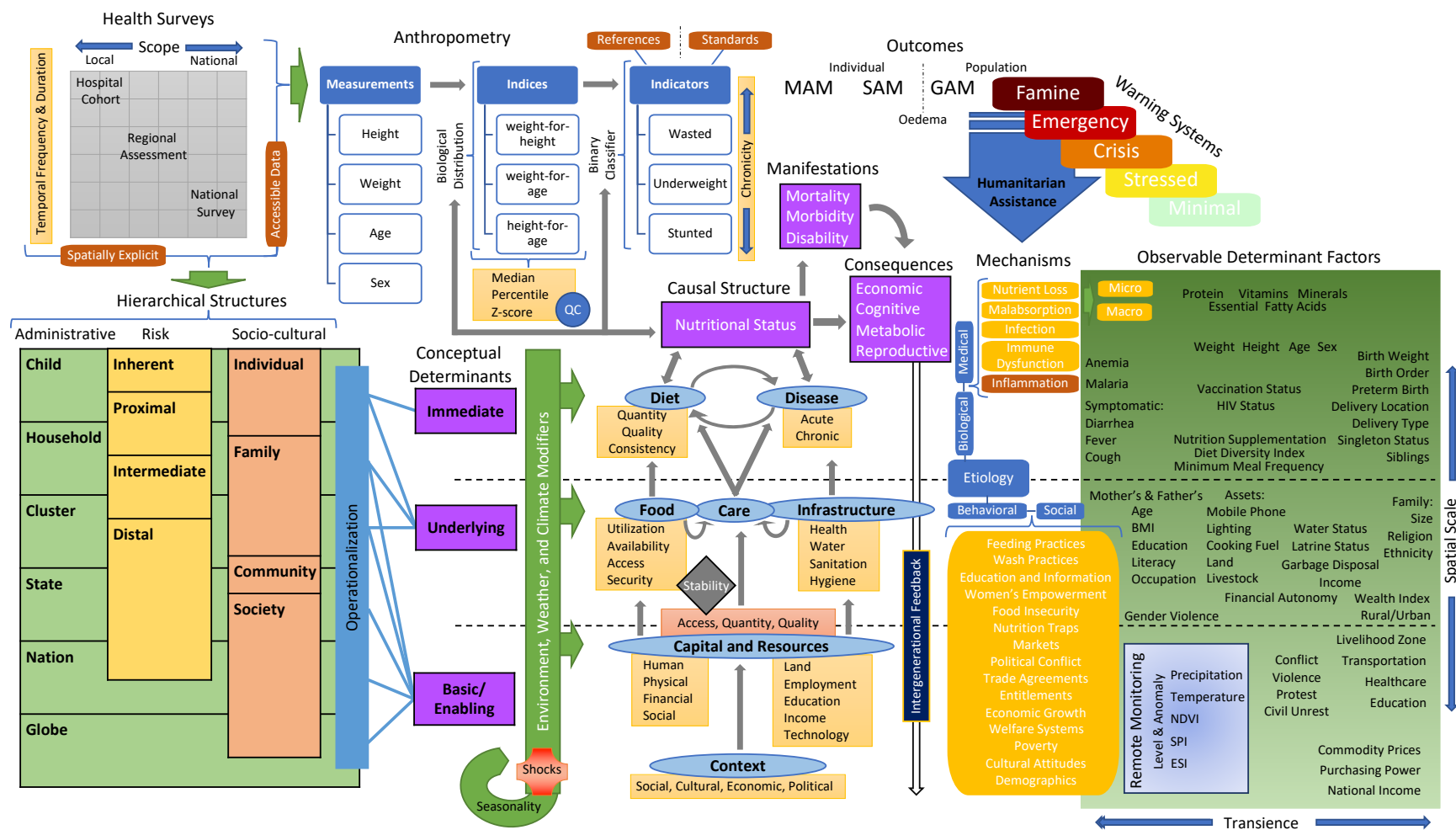


Figure 1: Comprehensive conceptual framework

1.1 Objectives and Research Questions

Taking a leaf out of Svedberg (2000), I propose four principal challenges facing the field: 1) better characterizations of what undernutrition is; 2) more reliable estimates of who the undernourished are and their numbers; 3) a better knowledge of where the undernourished are and their stochastic nature over time; and finally 4) a better epistemological understanding of how the undernourished get to be so. I address each of these four principal challenges across four standalone essays.

The first essay takes a historical and epistemological perspective on the science of anthropometry—the study of the measurements, indices, indicators, standards, and references used to assess child malnutrition. Anthropometric evaluation of children is the most vital and widely used instrument of public health and clinical medicine practitioners. I chronicle the legacy of standardization, normality, and eugenics in the study of child anthropometry.

I ask: What are the origins of anthropometry as a scientific study? How do these origins impact contemporary scientific anthropometry? How did the contemporary measurements, indices, indicators, standards, references, and best practices come to be? What are the existing practical and epistemological limitations and what are the areas ripe for further development? How does population-based anthropometric indicators translate to knowledge and improvement of an individual's health.

The second essay is a systematic review of the malnutrition literature. Specifically, I review and synthesize 184 articles encompassing the totality of

disaggregate empirical studies of the determinants of childhood malnutrition in Africa published since 1990. It is by far the largest and most comprehensive of its kind.

I ask: What group of articles constitutes the total populace of disaggregate empirical studies of the determinants of child malnutrition in Africa published since 1990? What are the findings of these articles? What are the trends in place, space, and scope across time? What are the trends in methodological techniques? What are the trends in choices of indicators, risk factors, and predictors? What are the spatial, hierarchical, and temporal frameworks? What are the trends across the journals? Where does the data come from and what is the data structure and size? What are the determinants of malnutrition? How much do the determinants affect malnutrition? What articles stand out for their high impact or quality?

The third essay is a rhetorical perspective and analysis of the quality control mechanisms for anthropometric surveys. Once an anthropometric survey of child malnutrition is conducted it is essential to know and assess its quality if we wish to quantify determining factors, test hypotheses, and inform policy in the effort to eliminate child malnutrition. I dispute the practice of performing quality control verification of anthropometric surveys using sample z-score standard deviations.

I ask: What are the origins of a z-score standard deviation serving as a quality control metric? What is the extent and impact of the SD as QC maxim in the literature? What are the theoretical and logical underpinnings of the practice? What harm is caused by the practice? What are the potential alternatives?

The fourth essay is a spatial and econometric empirical analysis of malnutrition determinants. The objective of the fourth essay is to analyze the determinants of

childhood malnutrition in Kenya and Nigeria. I uncover important climatic and economic determinants of child malnutrition, and quantify their epidemiological significance.

I ask: In Kenya and Nigeria, what are the determinants of wasting and stunting? How much do the determinants affect malnutrition outcomes? In particular, how effective are remotely sensed climatic variables for determining malnutrition prevalences? What are the spatial trends and variations of wasting and stunting? How much does spatial heterogeneity and hierarchical systems explain the variability in malnutrition outcomes?

1.2 Key Outcomes

In the first essay, I identify a gap in the literature of histories of scientific thought, and the legacy of anthropometry as a science in particular. There is an unmet need for decolonization of the literature, and an examination of the current concomitant practices. Through historiographical means and archival processes, I uncover a nonlinear and contested record of events, up to and including leading contemporary practices and datasets. I contextualize the legacy of child malnutrition studies in a broad framework, including the linkage between the early eugenics movement and contemporary notions of a “normal” child, the interpersonal and institutional rivalries to develop the preeminent child growth charts, the often overlooked distinction between reference growth charts and standards of growth, and the hitherto unexplored consequences of universal growth standards, which fail to reflect actual population characteristics.

In the second essay, I find a mismatching of studies with malnutrition severity: looking only where the light shines brightest. I show that there is a disproportionately large focus on stunting, and little focus on spatial analysis, quantification, and interpretation of results. Large but imprecise (statistically insignificant) variables are left ignored, while small but precise (statistically significant) variables are touted as policy relevant areas of focus.

In the third essay, I critique the standard deviation quality control maxim of anthropometric survey indicators: essentially, to dismiss any survey of anthropometric measurements whose standard deviation exceeds that of a benchmark distribution. I detail the genesis and propagation of the maxim in the literature, expose its theoretical and logical weaknesses, illustrate its demerits, and offer an alternative approach.

In the fourth essay, I show the distinctiveness of places juxtaposed to the regularities within and between these places. I measure possible outcome effects of policy changes that target specific social determinants and various climate scenarios. The contribution to the literature is twofold: first I combine climactic data with the social determinates of health in a spatially explicit and quantifiable, epidemiologically significant framework; and second, I account for possible variability between the primary sampling units in the form of a four-level hierarchical model (generalized non-linear mixed model), the first of its kind.

2 The Chronicles of Anthropometry: The Legacy of a Standard of Normality in Child Nutrition Research

2.1 Overview

Anthropometric evaluation of children is among the most vital and widely used instrument of public health and clinical medicine. Anthropometry is used for establishing norms, identifying variations, and monitoring development. Yet the accurate assessment of physical growth and development of children remains a perpetually beleaguering subject. This paper focuses on the evolution of anthropometry as a science and its associated measurements, indices, indicators, standards, references, and best practices. This paper seeks to clarify aspects of the assessment of child growth, explores the historical trajectory of the study of anthropometry and its contemporary limitations, and contributes to the debate surrounding references and standards, and the applicability of international anthropometric standards to an individual's health. It contextualizes the legacy of child malnutrition studies in a broad framework, including the linkages between eugenics and contemporary notions of "normal" child growth, the long contested institutional power-struggle for child growth chart superiority, the obfuscated distinction between growth reference and standards of growth, and the unforeseen consequences of universal standards that do not reflect any observable populations.

2.2 Introduction

Anthropometry is the scientific study of the measurements and proportions of the human body. The World Health Organization asserts, “that for practical purposes anthropometry is the most useful tool for assessing the nutritional status of children” (WHO, 1986, p. 929). Other approaches to measure malnutrition include self-reported hunger levels and estimates based on food supply, however, they are less reliable (Svedberg, 2011). Child malnutrition is an indicator of food and nutrition security (Smith et al., 2000). Although anthropometry is not the same as health, it is significant and useful for understanding health (Komlos, 2009). There is little reason to doubt the importance and urgency of improving child health and nutrition, substantiated by a resolute anthropometric method.

In general contemporary terminology, the basic anthropometric *measurements* are age, sex, weight, and height. Other measurements include subscapular skinfold thickness, triceps skinfold thickness, mid-upper arm circumference, and head circumference. An *index* is a combination of measurements (e.g., weight-for-height, height-for-age). They are necessary for grouping and interpreting measurements. The most prominent anthropometric index expression is the *z-score*. It is derived from the difference between a particular child’s weight-for-height or height-for-age and the comparable value from a reference population, divided by the standard deviation of that reference population (WHO, 1995). The most ubiquitous growth chart is the 2006 WHO Child Growth Standards (Natale & Rajagopalan, 2014).

An *indicator* is the application of an index prescribing judgement on the health of an individual (e.g., wasted, stunted, underweight). An index is a numerical

calculation only, whereas an indicator is a value based grouping or cutoff (WHO, 1986). The two most widely studied contemporary indicators are *wasting* and *stunting*. Wasting indicates a deficit in tissue and fat mass, either from weight loss or inability to gain weight. Stunting indicates impeded skeletal growth. It is an evaluation of linear growth, representing chronic malnutrition accumulated over time. Nutrition monitoring and intervention programs hinge on specific, accurate, and standardized indicators (UNICEF, 2013).

Why are these the dominant accepted paradigms and how did they get to be so? Historians of science know that understanding how and why a science (in this case anthropometry) developed its methods and gained its prominence raises profound questions. Social context, metaphysical assumptions, professional aspirations, and ideological allegiances are significant to the histories of a science. A conventional and sanitized history of science—which ignores blind alleys, errors, and distortions in the past—is incomplete. This paper attempts to grapple with some of these unconventional and ignored questions, particularly questions pertaining to the evolution and prominence of universal growth charts, the lasting impacts of emphasizing “normal” children, why and how categories of healthy growth developed and who was responsible, the oft-ignored distinction between references and standards, and the unforeseen consequences on the applicability of recommendations of child growth derived from universal growth standards.

Broadly speaking, the present article consists of seven compound objectives. The first section chronicles the nascent development of the science of anthropometry, detailing the motivations and findings of contributors to the field at its inception. The

second section introduces the premise that the motivations and findings of anthropometric science is inextricably linked to the eugenics movement and how the notion of a “normal” child (described later as still in contemporary practice) derives from this doctrine. The third section chronicles the development of child growth charts and the struggle of various institutions to supplant one another as the preeminent authority, leading to a movement away from regional and national tables towards a single unified international reference. The fourth section traces the semantic evolution of methods and terminology of anthropometric measurements, indices, and indicators to describe child malnutrition in its various forms. This section also explores the struggle between quantitative and qualitative classifications, and juxtaposes the needs of cold statistical objectivity against individual subjective judgement and evaluation. The fifth section examines the distinction between reference growth charts and standards of growth, the continued development of unified international growth charts, and what it means to be a “normal” child. The sixth section highlights the origins for ongoing debates of the social determinants of health and the meta-histories of anthropometry. The final section analyzes the state of the contemporary preeminent international child growth chart derived from the 2006 WHO Multicentre Growth Reference Study.

This framing reveals a picture of anthropometry as a cultural product and a political resource. As Rudwick puts it, “Accepting or rejecting any scientific theory is always and irreducibly a social act, by a specific social group, in particular cultural circumstances” (1981, p. 247). Demonstrating that anthropometry has always been contested and negotiated, this historical awareness helps to keep the subject open to

dialogue and debate. Future policies and initiatives will be more effective and successful if they are shaped against a background that includes an understanding of the forces and factors of past developments.

2.3 A Nascent Science

The genesis of anthropometry is not in medicine or even science, but in the arts and Pythagorean philosophy (Tanner, 1981). It was sculptors and painters, in search of Platonic ideals, who first measured the relative proportions of the human form. The nascent scientific study of measurements and proportions of the human body was conceived by Adolphe Quételet. The Quételet Index, later redubbed the Body Mass Index, is still relevant (Eknoyan, 2008). In his 1832 article *Research on the weight of man at different ages*, Quételet describes the first cross-sectional study of the height and weight of newborns and children (Quételet, 1832). In his 1835 text *A treatise on man and the development of his faculties*, Quételet presented his conception of the “average man” and the link between the population distribution of weight and height to the normal Gaussian distribution (i.e., a bell curve) (Quételet, 1835).

It was not until after the UK Parliament passed the 1833 Factory Act, reforming inadequate child labor standards in factories, that a need arose for physicians to measure and standardize the growth rates of children. The Act required physicians to certify children’s “age and physical capacity for work ... and that the [child] has the ordinary strength and appearance of at least 8 years of age” (Roberts, 1876, p. 681). Following the passage of the Act there was a smattering of studies measuring the weight and height of children in select factories. However, it was Roberts (1876) who first endeavored to establish standards of reference for the height

and weight of children, collecting measurements from 10,000 boys and girls, aged 8 to 14, across urban and rural populations, and factory and non-factory households.

In 1883, the *Final Report of the Anthropometric Committee of the British Association for the Advancement of Science* was published. The Committee was appointed in 1875 “for the purpose of collecting observations on the systematic examination of the height, weight, and other physical characters of the inhabitants of the British Isles” (Galton, 1883a, p. 1). Under the chairmanship of Francis Galton (inventor of correlation and regression, and cousin of Charles Darwin), the Committee collected anthropometric measurements from 917 infants and 651 children under 5 years of age to construct tables of average weight and height. The primary questions of research at the time were concerned with developing general principles of growth and development, understanding the link between social class and mental and physical capacity in children, and discerning the point at which growth matures (Burk, 1898). Similar efforts were also underway in the US (Bowditch, 1877). By the end of the 19th century interest in anthropometry—specifically anthropometry of children—was accelerating. Hartwell (1893) chronicled 117 titles of anthropometric works in the US. In 1898, Burk published growth curves and a study describing the “average” American boy and girl, based on the anthropometric surveys of Boas (88,449 Boston, St. Louis, Milwaukee, Worcester, Toronto, and Oakland children), Bowditch (24,500 Boston children), Peckham (9,600 Milwaukee children), and Porter (34,500 St. Louis children).

2.4 A Pure and Normal Child

In 1909, Ellen Key's *The Century of the Child* was published in English. The volume and its title served as spark and slogan for a burgeoning child welfare movement, which was gaining moral and political authority throughout Western Europe and the United States at the turn of the 20th century (Cravens, 1993). Key's message certainly resonated in the United States, especially with people like Cora Hillis of the National Congress of Mothers (the progenitor of the National Parent Teacher Association), who in 1917 fought to establish the Iowa Child Welfare Research Station.¹ The Research Station pioneered methods of assessing children's nutritional status with anthropometry indicators in order to "give the normal child the same scientific study by research methods that we give to crops and cattle" (Bradbury & Stoddard, 1933, p. 7). It was there that the notion of a "normal" child was championed.

However, the notion of a "normal" child and the study of anthropometry is inextricably linked to the early eugenics movement. It was Francis Galton himself who coined the term eugenics as "the science of improving [human] stock [through] judicious mating ... to give the more suitable races ... a better chance of prevailing over the less suitable" (Galton, 1883b, p. 25). The early child wellbeing researchers assumed the national population was divided into a hierarchical series of groups, some superior and some inadequate, with native-born whites of Anglo-Saxon Protestant ancestry at the top (Cravens, 1993).

¹ Hillis's first appeal for a research station at Iowa State University was dismissed because "the college's mission was pigs, not people." And her request in 1915 from the Iowa Legislature for \$25,000 to establish the research station at the University of Iowa was denied in favor of a new sheep barn built at the state fairgrounds.

Ellen Key, echoing Galton, called for “very strict rules, to hinder inferior specimens of humanity from transmitting their vices or diseases, their intellectual or physical weaknesses” (Key, 1909, p. 20). Fully in the mainstream of her time, Cora Hillis also campaigned for racial purity in order to promote the Research Station (Cravens, 1993). Anthropometry has been conjoined since its inception as a scientific practice with the ideals of eugenics. Despite meaningful insights from anthropometry, this legacy has beset the field.

From these early studies medical professionals began to use height-weight-age tables as an index of child health and as a measure of severe malnutrition, replacing the inadequate measure of weight only (JAMA, 1933). The impetus for an index of child health came from the Baldwin-Wood tables, first published in 1910 and revised in 1923, which soon became widely taught and reproduced in most textbooks (Tanner, 1952). Emerson and Manny (1920) first proposed a normal zone—of 7% below to 20% above average weight for height—to identify malnourished children, determining that 20 to 40 percent of US children were malnourished. Accompanying the salutary results of the research, interpreting the limits of the normal zone was generally misunderstood by anxious parents who would consult oracular weighting machines to gauge their child’s health (Tanner, 1952). Even medical professionals misunderstood and trivialized malnutrition, dominated instead by the ideas of infection (Williams, 1973). But unlike infection, which asks the qualitative question “Whether or Not” (a child is infected), malnutrition asks the quantitative question of “How Much” (a child is malnourished).

2.5 Growth Curve Standardization and Unification

By the early 1940s the study of *velocity of growth* grew in prominence. First advocated by Frank K. Shuttleworth, he deemed cross-sectional data inadequate for all meaningful analysis with the exception of “determining the average size of children in general at any given age” (Shuttleworth, 1937, p. 180). However, determining velocity was financially, administratively, and computationally burdensome, requiring longitudinal rather than cross-sectional studies (Tanner, 1952). Boas (1892) realized the importance of longitudinal data, but was largely ignored until 40 years later when he clarified the statistical and scientific gains to be had from following individuals through time (Boas, 1930).

The first longitudinal charts came from studies in the United States, consisting of 50 to 200 children from homologous communities (Bayer & Gray, 1935; Jackson & Kelly, 1945; Palmer et al., 1937; Palmer & Reed, 1935; Robinow, 1942; Simmons & Todd, 1938; Wetzel, 1941). Older studies and charts did exist in a sense. As far back as 1872 Bowditch collected longitudinal data; however, he only studied 13 girls and 12 boys who were all mostly related and older than 5 years of age (Bowditch, 1877). These studies, however, were only quasi-longitudinal, with many children only being observed for a few years at a time. Despite their shortcomings, these standards of reference would not be fully supplanted until 2000 to 2006 (de Onis, Garza, et al., 2007).

In a perpetual trend that continues today, the accepted standards of anthropometric measurement continued to evolve. Growth rate norms developed from data earlier than the 1930s (i.e., the Baldwin-Wood tables) were deemed inadequate

for evaluation. Critics like Shuttleworth (1934) decried the inadequacies of the contemporary standards of development. Pointing to the secular trend over the past century towards heavier and taller populations (see Roberts, 1876), previous standards of reference were quickly deemed out-of-date (Meredith, 1941; Meredith & Meredith, 1944; Tanner, 1952). The secular growth trend debate continues to beleaguer contemporary studies of anthropometry (NCD-RisC, 2017).

Stuart and Meredith (1946) provided the first such updated standards, collected from 750 children between the ages of 5 to 18 years of “northwest European ancestry living under better than average conditions from the standpoints of nutrition, housing, and health care” at the Iowa Research Station (Meredith, 1949, p. 884). In the fifth edition of *Mitchell-Nelson’s Textbook of Pediatrics* (for the past 70 years the most prominent book of its kind), Stuart and Stevenson (1950) provided further updates from the Harvard School of Public Health Longitudinal Studies data, including children from birth to 18 years old. These anthropometry standards—referred to as the Harvard-Iowa standards—remained in prominent use for the next thirty years (Tanner, 1981). Similar efforts were also underway in the Netherlands (de Wijn & de Haas, 1960) and Britain (Tanner & Whitehouse, 1959).

Despite its prominence, the Harvard-Iowa standards were recognized as inadequate for a national reference, much less for an international reference, but such is the effect of professional prestige and political power. In an effort to standardize inadequate nutrition assessments, the World Health Organization in 1966 published a simplified combined-sexes version of the Harvard-Iowa standards (Dibley, Goldsby, et al., 1987). Certifying itself as exemplar, the World Health Organization established

methods, techniques, and procedures for defining, collecting, presenting, and interpreting anthropometric measurements (D. Jelliffe, 1966). Pediatricians and public health officials were beginning to adopt anthropometry and children's health as a sensitive index of the health of a community (Tanner et al., 1966). Indeed, the Assistant Director-General of the World Health Organization, W. H. Chang proclaimed, "Health of a population is reflected most accurately by the rate of growth of its children" (Eveleth & Tanner, 1976, p. ix).

In 1967 the World Health Organization and UNICEF (United Nations International Children's Emergency Fund) collaborated with the International Biological Programme (under the auspices of the International Council of Scientific Unions) to collect anthropometry data from a globally representative sample spanning 42 countries and 340 projects, in an unprecedented multilateral effort, including a joint longitudinal study of children in Paris and London, to serve as the new reference (Eveleth & Tanner, 1976). Unfortunately, the efforts of the International Biological Programme lacked traction in the nutrition sphere and became defunct by 1972.

The First Joint Food and Agriculture Organization/World Health Organization Committee on Nutrition convened in 1949. In keeping with its persistent message, the First Expert Committee prescribed a need for studies of the clinical characteristics of early childhood malnutrition (FAO & WHO, 1949). Under the United Nations' collective belief that health is a fundamental human right and the healthy development of children is of central importance, nutritional needs assessments in underdeveloped countries began in earnest. By 1971, the Eighth Expert Committee prescribed a need to study incidence and prevalence of malnutrition, and the urgent

prerequisite of a general consensus of definitions and classifications. They also highlighted other concurrent issues such as the etiology of malnutrition and role of non-illness (socio-economic) factors, and the permanent physical and mental impairment caused by malnutrition (FAO & WHO, 1971). Greater understanding of the mechanisms of malnutrition, highlighted by Emerson and Manny (1920), spurred by Jelliffe (1966), and underscored by Waterlow (1972), led to the supremacy of height-for-age and weight-for-height anthropometric indices, supplanting the inadequate weight-for-age index (Waterlow et al., 1977; WHO, 1976).

Perpetuating the discourse of ever more rigorous standards, the Maternal and Child Health Program, the United States Public Health Service, and the American Academy of Pediatrics concurred in 1971 that the Harvard-Iowa standards were inadequate and no longer applied to the US (Hamill et al., 1979). This decision was the impetus for the Health and Nutrition Examination Survey carried out by the Centers for Disease Control and Prevention's National Center For Health Statistics Task Force and later recommended by the US National Academy of Science in 1974 as the new US national anthropometric reference (WHO, 1978).

First released in 1977, the National Center For Health Statistics Growth Curves were a combination of data from the National Center For Health Statistics' Health Examination Surveys, Health and Nutrition Examination Survey and the Fels Research Institute (Hamill et al., 1979). The National Center for Health Statistics data consisted of three pooled quasi-longitudinal surveys (1963 to 1974) measuring the anthropometry of 2 to 18 year-olds from a national stratified probability sample (Hamill et al., 1977). The Fels data was compiled from a sample of convenience of

867 white middle-class Ohio children during a longitudinal study (1929 to 1975) of children from birth to 3 years old (Dibley, Goldsby, et al., 1987). The portmanteau quality of the growth reference led to a discontinuity at the junction point of the disparate data sets (Dibley, Staehling, et al., 1987). The discontinuity produced spurious interpretations of anthropometric indicators, which incorrectly implied a drop in prevalence rates at 2 years old. This spurious artifact persists today in many studies on the etiology of malnutrition.

Waterlow et al. (1977) of the World Health Organization described the canonical criteria for an anthropometric reference population, which would establish the US National Center for Health Statistics Growth Curves (Hamill et al., 1979) as the preeminent growth reference for both individuals and populations for the next 30 years. In 1978 the Centers for Disease Control and Prevention developed a statistically normalized version of the National Center for Health Statistics Growth Curves (Dibley, Goldsby, et al., 1987). In the same year the World Health Organization adopted the normalized Growth Curves and succeeded in promoting them as the preeminent international growth reference. The single international reference population allowed pediatricians, public health officers, and organizations like the World Health Organization to compare the results among different nutrition studies, assisting interpretation and improving clarity (WHO, 1978).

2.6 Categories, Cutoffs, and Classifications

Though not the first to try, Waterlow et al. (1977) cemented normalized growth charts and z-scores as the definitive indicator measurement. The most common expressions of anthropometric indices are *percent-of-median*, *percentiles*, and *z-scores*

(sometimes referred to as standard deviation scores) used to group and interpret measurements. Percent-of-median is the ratio of an anthropometric measurement or index for a child (e.g., their weight) to the median value of comparable children in the reference population, expressed as a percentage (WHO, 1995). Percent-of-median is the simplest to calculate and a useful measurement if the distribution of the reference population is unknown, unspecified, or otherwise not normalized (Gorstein et al., 1994).

Percentiles rank the relative position of a child against comparable children in the reference population, expressed in terms of what percentage of the reference population the child equals or exceeds (WHO, 1995). Percentiles are the most intuitive, and formerly the most common way physicians tracked a child's growth; the 50th percentile or the *median* (and if the reference is perfectly Gaussian normal, also the *mean*), describes the central point with 50% of the population above it and 50% of the population below it (Falkner, 1962).

Z-scores convey anthropometric measurements as a number of standard deviations below or above the reference population value. Z-scores are the difference between a child's measurement and the mean value of comparable children in the reference population, divided by the standard deviation of the reference population (WHO, 1995). Z-scores require a reference population that follows a normal (Gaussian) distribution. In return, z-score cutoff values are stable across different reference populations (e.g., defining a -2.0 weight-for-height z-score as wasted is consistent across all heights and even through other conditional factors such as age). Z-score measurements are also useful for comparing measurements across different

units (Falkner, 1962), and as a feature of normalization the full distribution of anthropometric values can be expressed with just a mean and standard deviation. Z-scores are now accepted as the best system for analysis and presentation of anthropometric data (de Onis & Blössner, 1997; de Onis & Habicht, 1996; WHO, 1995).

The terminology used to describe malnutrition has gone through many renditions. As one anonymous author in the *British Medical Journal* once said: “All we can demand is ... that language shall not lag behind knowledge; and that, as we learn to know things better, we shall also take due pains to name them more perfectly” (Anonymous, 1886, p. 1116). Etymologically speaking, the terms *wasting* and *stunting* are ideophones: purely descriptive of the symptomatic thinness and shortness of malnutrition.

As early as Emerson and Manny (1920), *stunting* described low height-for-age whereas *malnourished* described low weight-for-height. At the First Joint Food and Agriculture Organization/World Health Organization Committee on Nutrition *kwashiorkor* or *malignant malnutrition* was the watchword of the day (FAO & WHO, 1949). *Kwashiorkor* is a Ghanaian word meaning “the disease of the deposed baby when the next is born” (Williams, 1973, p. 361). First described by distinguished pediatric pioneer Cicely Williams (1933), it is a type of clinical malnutrition from deficient protein intake coupled with *edema* (i.e., an excess of fluid in body tissues and cavities). By the Third Joint Committee, the nutrition lexicon shifted to *protein-calorie malnutrition* and included descriptions of “wasted muscles” hinting at the ensuing terminology (FAO & WHO, 1953).

During the intervening decade, 1950-1960, the field of nutrition shifted emphasis from *micronutrients* (vitamins A and B, iodine, and zinc) to *macronutrients* (proteins, fats, and carbohydrates) (Jolliffe, 1962). Jelliffe (1966) suggested the term *protein-calorie malnutrition of early childhood* should be used as a generic term to cover the whole range of manifestations, which would include the clinical syndromes of *kwashiorkor* and *marasmus*—a more general form of starvation with signs of “severe wasting,” but not *edema*. He also distinguished between four forms of malnutrition: *undernutrition*, *specific deficiency*, *overnutrition*, and *imbalance*. In modern parlance, “severe acute malnutrition” and “severe wasting” have superseded *kwashiorkor* and *marasmus* (WHO & UNICEF, 2009).

Waterlow (1972) proposed *retardation* as the slowing of linear growth where *stunting* would describe a reduction in final stature. Following Seoane and Latham (1971), who noted weight-for-height gauges *current* nutrition and height-for-age gauges *past* nutrition, Waterlow (1972) also proposed four categories of nutritional status: normal; malnourished but not retarded (*acute malnutrition*); malnourished and retarded (*acute on chronic malnutrition*); and retarded but not malnourished (so-called *nutritional dwarfs*). Each category was accompanied with a grade to further distinguish the severity. By 1977, the contemporary derivations of *wasting* (low weight-for-height) and *stunting* (low height-for-age) were established.

But the *sorites* problem—the ancient Greek paradox of how many grains of sand it takes to make up a heap—remained unresolved. That is, at what point is a child stunted, wasted, underweight, malnourished or severely malnourished? Determining a child’s nutritional status based on anthropometric values requires

defining cut-off points, which needs a qualitative classification, whereas prevalence and severity needs a quantitative classification (Waterlow, 1972). To use Stevens's (1946) typology of scale, one must transform a *ratio* measurement into a *nominal* grouping.

Using weight-for-age, Gómez et al. (1956) imposed explicit cut-off points (i.e., 76-90, 61-75, and less than 60 percent-of-median) to classify malnutrition severity into *first degree*, *second degree*, and *third degree malnutrition*. Ford (1964) suggested that 66 percent-of-median should be the malnutrition line. Garrow (1966) proposed that severe malnutrition occurred only below 70 percent-of-median weight-for-age. Dugdale (1971) believed malnutrition began at 80 percent-of-median reference weight. Waterlow (1972) tweaked the Gómez Classification; using weight-for-height he suggested three delineated malnutrition severities of 90-80, 80-70, and less than 70 percent-of-median. Trowbridge (1979) classified wasting as below 80 percent-of-median and stunting as below 82.5 percent-of-median. The Oomen Malnutrition Index (Oomen, 1955) and Protein-Calorie Malnutrition Score (Jelliffe & Welbourn, 1963) were other attempts to establish a common system, but the Gómez classification is considered the progenitor of the modern malnutrition classification system (de Onis, 2000; D. Jelliffe, 1966). Originally the Gómez classification was devised to group cases of similar prognosis for children aged 1 to 4 years and guide physicians in selecting the appropriate place of treatment. It was not intended as a diagnostic classification tool for community surveys nor to be extended to other age groups (FAO & WHO, 1971; Gómez et al., 1956).

With the increasing prominence of normalized curves and z-scores, Waterlow et al. (1977) defined the contemporary canonical cut-off points for moderate wasting and stunting as 2 standard deviations below the median reference, and for severe wasting and stunting as 3 standard deviations below the median reference (UNICEF, 2013). Though largely ignored, the Eighth Report of the Joint Food and Agriculture Organization/World Health Organization Expert Committee on Nutrition did warn against the problem of a “normal” standard in tests of nutritional status (FAO & WHO, 1971). “In most biochemical and haematological measurements it is usual, for practical reasons, to specify ranges and “cut-off” points that distinguish “normal” individuals or groups from those who are “at risk” or “deficient”” the Report goes on to say, “This is an arbitrary procedure, since most parameters vary continuously ... [and statistical evaluation] cannot by itself distinguish between what is normal and abnormal in the biological sense” (FAO & WHO, 1971, p. 76). Sole reliance on statistical evaluation continues, with little consideration as to the sensitivity and specificity of an arbitrary cut-off point.

2.7 References to Standards

Using the 1978 normalized Growth Curves, the World Health Organization continued to collect and publish (in 1983, 1989, and 1993) information on the nutritional status of the world’s children (de Onis & Blössner, 1997). In 1986, a World Health Organization Working Group published a conclusive guide to define, interpret, and standardize anthropometric indicators (WHO, 1986). By 1993, the Expert Committee on Physical Status, convened by the World Health Organization, concluded that despite previous admonitions, *reference* growth charts had long been misconstrued as

a *standard* of growth (de Onis & Habicht, 1996). The National Center for Health Statistics and the Centers for Disease Control and Prevention designed both the 1977 smoothed percentiles and the 1978 normalized growth curves as *references* (Kuczmarski et al., 2002).

The sole aim of a *reference* is to be a common basis in order to group, analyze, and compare different populations, whereas a *standard* represents a desirable target or norm (WHO, 1995). In practice, however, clinicians use growth charts as *standards* rather than *references* (Grummer-Strawn et al., 2010). The distinction may seem trivial, but the requirements of the underlying data will change depending on the intended application, which can produce spurious interpretations and conclusions.

The problem is also circular. To be able to identify the normal range in a population the abnormal ones must first be removed, but abnormalities can only be identified once the normal range is defined (Armstrong, 2019; Creadick, 2010; Rose, 2016). Not to mention the well documented paradox that given enough measurement dimensions—even a small number of dimensions across a homogenous sample—exactly zero people will be “average” (Creadick, 2010; Rose, 2016; Subramanian et al., 2018). However, the question remains of whether it is appropriate to compare children across radically different environments, and whether the *reference* versus *standard* distinction is satisfactory or merely evades the larger issue (de Onis & Blössner, 1997).

Clearly, different subpopulations have different proclivities for growth, based on their environment, gene pools and the interaction between the two (Eveleth & Tanner, 1976). However, Habicht et al. (1974) believed that standards which

represent optimal growth can apply to all children, regardless of race or ethnicity, because their potential effects are so small compared with environmental effects. Contemporaneously, Waterlow et al. proclaimed that, “If there were differences dependent on different gene distributions, then the target for one population would not be the same as the target for another. This does not, however, affect the use of the reference data for comparisons between populations” (Waterlow et al., 1977, p. 490).

Tempting as it may be, the desire to distill all observed differences in human growth and behavior down to the environment and gene pools should be avoided, especially if accompanied by a numerical ranking, echoing eugenics and environmental determinism.

Even the canonical arbiters of the international anthropometric reference conceded that:

Because the reference population cannot be used as a universal target, the question of what is a realistic goal in any particular situation does become important. Decisions of this kind have to be taken locally, and it is not possible to make international recommendations about them. (Waterlow et al., 1977, p. 490)

The distinction was, and continues to be, largely overlooked.

In constructing the international growth reference chart, the National Center for Health Statistics decided that smoothed growth curves looked better and represented reality better. Although mathematical smoothing techniques have long existed, the 1977 reference was the first to use computers to systematically smooth its curves in a reproducible, quantifiable way (Hamill et al., 1977). The result produced artificial growth curves in order to serve statistical techniques of comparison that depend on the normal (Gaussian) distribution (Dibley, Goldsby, et al., 1987). The

increasing normality of the international reference data (in the statistical Gaussian sense), however, exacerbated the phenomenon of misapplying the reference as a standard (WHO, 1995). Recognizing this phenomenon along with other inadequacies of the data (e.g., discontinuities and unrepresentative samples of convenience) led to the development of new growth charts, which purported to serve as both reference and standard.

2.8 Histories, Etiologies, and Determinants

Pioneering the research on the social causes of malnutrition, José María Bengoa (1940) believed malnutrition to be an ecological problem: the result of overlapping factors in a community's physical, biological and cultural environments. Physician Norman Jolliffe (1962) proposed a twofold classification for the pathogenesis nutritional deficiency. Jolliffe's classification places a faulty diet as the primary cause, which is conditional upon inadequate or abnormal nutrient ingestion, absorption, utilization, and excretion. This etiology is firmly couched within the purview of illness related malnutrition (Mehta et al., 2013).

Moving towards a non-illness etiology of the social determinants of health, tropical pediatric expert Dr. Derrick Jelliffe (1966) proposed that the principle aim of nutritional assessment should be to map out the magnitude and geographical distribution of the problem and analyze the direct and indirect ecological factors. The *entitlements* paradigm, conceived by Nobel Prize-winning economist Amartya Sen (1976), approached the study of poverty and hunger by illuminating the less than obvious economic mechanisms when dealing with less than extreme raw poverty and its antecedents.

The same year Sen devised entitlements, physician and demographic historian Thomas McKeown (1976; 1979) proposed that economic growth coupled with better nutrition (i.e., greater caloric intake) caused improvements in health outcomes, rather than targeted public health or medical interventions. Dubbed the “McKeown thesis,” it became the subject of much controversy and shaped the research hypotheses of many scholars (Colgrove, 2002).

Motivated by McKeown and coinciding with the search to develop child growth standards, the National Bureau of Economic Research conducted numerous early studies on anthropometric history and trends (Cuff, 2019). In the late 1970s researchers such as Nobel Prize-winning economist Robert Fogel began to create the new anthropometric history (Steckel, 2009). The founders of this newly developing interdisciplinary perspective were instrumental in bridging child growth and economic development, and connecting components of biological welfare with the socioeconomic and epidemiological environment during childhood (Komlos & Baten, 2004). In particular, anthropometric history found a niche in scholarship by incorporating the effects of environmental externalities, cyclical fluctuations, family resource distribution, societal level inequalities, and spatial disparities from historical records (see Floud & Wachter, 1982; Fogel et al., 1982; Fogel et al., 1978; Friedman, 1982; Komlos, 1985, 1998; Margo & Steckel, 1982, 1983; Sokoloff & Villaflor, 1982; Steckel, 1979; Tanner, 1982; Trussell & Steckel, 1978).

Much of McKeown’s particular arguments about public health have been largely invalidated, but the legacy remains. Stiglitz (1976), picking up where Leibenstein (1957) left off, argued productivity depends (nonlinearly) on nutrition

from an efficiency wage perspective. Szreter (1988) argued that public health measures—especially clean water and improved sanitation—fundamentally reduces mortality and causes improvements in health outcomes throughout history. While others, such as Behrman and Deolalikar (1987), Bouis and Haddad (1992), and Bouis (1994), proposed that increases in income will not result in substantial improvements in nutrient intake, from an Engel curve for calories perspective. However, Subramanian and Deaton (1996) argued calorie elasticity is not zero, suggesting sufficient daily calories can be readily purchased with only a small fraction of the daily wage.

Fogel (1994; 2004) documented direct evidence for the importance of nutrition, connecting levels of calorie availability to their effects on health throughout history. He postulated that understanding *nutrition traps* is the key to both improved health and economic development. Smith and Haddad (2000), from an aggregate cross-county perspective take the broader view, suggesting the main determinants of malnutrition are national income, poverty, education, and the state of the health environment. Under the chairmanship of Jeffery Sachs, the WHO Commission on Macroeconomics and Health, suggested that good health is a necessary—and possibly sufficient—condition of economic growth, which suggests that improving health, and as a consequence stimulating economic growth, requires direct intervention through public health provisioning (WHO, 2001).

However, Deaton (2003) concluded that there is no direct link from income inequality to ill-health. Deaton goes a step further to emphasize the reinforcing interplay between disease and nutrition. He showed how nutrition traps are much

easier to understand once disease is given its proper place in the story. Malnutrition compromises the immune system, while at the same time, disease prevents the absorption of nutrients. For example, giving more food to a malnourished child afflicted with severe diarrhea would not ameliorate her health. As such, scientists, pediatricians, public health policy makers, and nutrition assistance programs need to carefully consider the many nuances of anthropometric modeling.

2.9 The New Normal

In 2000, the US Centers for Disease Control and Prevention released a revised version of the National Center for Health Statistics growth charts, and recommended them for both clinical and research purposes to evaluate the growth status of children in the US (Kuczmarski et al., 2002). These Growth Charts are based on five nationally representative surveys administered between 1963 and 1994 (de Onis, Garza, et al., 2007). The revised charts amended previous issues of discontinuity and unrepresentative samples, and an internal evaluation found no systematic differences between the smoothed and empirical data.

In a separate effort, the World Health Organization also concluded that the 1978 Growth Curves were inadequate (WHO, 2006a). As a result, the World Health Organization Multicentre Growth Reference Study was implemented between 1997 and 2003. The designers of the new Growth Reference were intentionally *prescriptive* rather than *descriptive* (i.e., they designed a reference for how children *should* grow rather than how children *actually* grow) (Garza & de Onis, 2004). In other words, it was purposely designed to produce a *standard* rather than a *reference*.

Despite the fact that the National Center For Health Statistics Growth Curves and the revised Centers for Disease Control and Prevention Growth Charts are a *reference*, whereas the World Health Organization Multicentre Growth Reference Study is a *standard*, there are those who propose to compare the two and recommend one as a universally better tool (de Onis, Garza, et al., 2007; de Onis et al., 2006; de Onis, Onyango, et al., 2007; Ziegler & Nelson, 2012).

Even as a standard, other studies find the Multicentre Growth Reference Study does not necessarily stand up (Bonthuis et al., 2012; Christesen et al., 2016; de Wilde et al., 2015; Heude et al., 2019; Júlíusson et al., 2011; Kêkê et al., 2015; Natale & Rajagopalan, 2014; Scherdel et al., 2015; Scherdel et al., 2016). The Standardized Monitoring & Assessment of Relief & Transitions (SMART) inter-agency initiative warns that,

The reference values should not be considered “ideal”; they are simply used as a standard to compare nutritional status in different regions, and in populations over time. It is a standard in the same way that the meter or the kilogram are standards used to measure length or weight. (SMART, 2006, p. 24)

Regardless, the Multicentre Growth Reference is the definitive international anthropometric “reference population.”

The Multicentre Growth Reference Study (July 1997–December 2003) consists of both cross-sectional and longitudinal surveys from six cities: Davis, California, USA; Muscat, Oman; Oslo, Norway; Pelotas, Brazil; select affluent neighborhoods in Accra, Ghana; and South Delhi, India (WHO, 2006b). The distributions of children across the different survey countries for the longitudinal component are: 119 USA; 149 Oman; 148 Norway; 66 Brazil; 227 Ghana; and 173

India. For a definitive global reference, the number of children the study is based on is rather small. The distributions of children across the different survey countries for the cross-sectional component are: 476 USA; 1,438 Oman; 1,385 Norway; 480 Brazil; 1,403 Ghana; and 1,487 India. Children were selected for inclusion based on: no known health or environmental constraints to growth, mothers willing to follow feeding recommendations (although only 20% actually did), no maternal smoking before and after delivery, single term birth, and absence of significant morbidity.

Of the 13,741 children screened for the longitudinal survey, less than 7% or 882 children (428 boys and 454 girls) were eligible, compliant, and included in the final study. In addition, of the 21,520 children screened for the cross-sectional survey, less than 31% or 6,669 children (3,450 boys and 3,219 girls) were eligible, compliant, and included in the final study. Notwithstanding the discontinuity problem seen in the 1978 Growth Curves, induced by a truncated longitudinal survey of children 0 to 24 months old, the longitudinal component of the Multicentre Growth Reference Study is an equally truncated survey of children 0 to 24 months old.

Prior to constructing the standards, if a child was 3 standard deviations above the sample median or 3 standard deviations below the sample median they were excluded (WHO, 2006b). For the cross-sectional sample the truncation procedure was even stricter. If a child was 2 standard deviations above the sample median or 2 standard deviations below the sample median they were excluded. In other words, even though the study sought out the healthiest, most ideal population to measure, 69-93% of the healthy populous (i.e., a very large percentage of the actual population) did not conform to this ideal (Sandler, 2021). As such, the Multicentre Growth

Reference Study is not representative of even a healthy population, much less a malnourished one.

The initial Multicentre Growth Reference Study sample was not a standard normal (Gaussian) distribution. After the selective sampling and exclusion exercise, the sample was exceedingly skewed to the right (WHO, 2006b). To rectify the non-normality, the data were cleaved at the median. The values from each new dataset were then reflected across the median to create two symmetrical distributions. Fitting a normal distribution to the data using the LMS method (Cole & Green, 1992), each mirrored distribution was used to derive standard deviation cut-off values for the respective upper and lower portions of the data.

This means that if describing a “population” effect or standard, most of the actual, non-statistical, real-world population distribution is fundamentally and structurally not represented. The population is a sum of individual identities and should provide a fluid denominator, comparator, context, and analytic space, yet now the population has come to define those very individuals (Armstrong, 2017).

Despite its shortcomings and checkered heritage, the Multicentre Growth Reference remains the most ubiquitous and authoritative resource of its kind (Natale & Rajagopalan, 2014). Even the United States Centers for Disease Control and Prevention (CDC), who develop their own specific child growth charts, “recommends that clinicians in the United States use the 2006 WHO international growth charts, rather than the CDC growth charts, for children aged <24 months” (Grummer-Strawn et al., 2010).

Only 47 countries have potential alternative growth charts to the Multicentre Growth Reference (Natale & Rajagopalan, 2014). Elsewhere in countries where child malnutrition is most severe and country specific child growth charts do not exist, the Multicentre Growth Reference remains the most relied upon growth chart of its kind. WHO contends that its growth curves describe how all children should grow in all countries and that any deviations from its standards should be considered as evidence of abnormal growth (Garza & de Onis, 2004; WHO, 2006b).

2.10 Conclusion

In the context of clinical nosology, Armstrong observed that “when classificatory systems and explanatory frameworks are in flux there is no Archimedean point from which to see things as they really are: neither causes nor reasons can have epistemological priority” (2011, p. 806). The statement aptly characterizes anthropometric evaluation as well. Chronicling the evolution of medical classification is rare and has not received the attention it deserves (Armstrong, 2011; Jutel, 2009).

Overlooking the legacy of a standard of “normality” in anthropometry could have profound consequences for contemporary etiological analyses of nutrition (e.g., Corsi et al., 2016; Kim et al., 2017; Kim et al., 2019; Perkins et al., 2017). To uncover its implications, we should continue to interrogate contemporary manifestations of anthropometric ontologies. It is well beyond the reach of this or any other single paper to disentangle the historical strands and perform this sort of examination, although it would not be impossible given more time and space.

3 A Scrupulous Review

3.1 Overview

Malnutrition devastates millions of children globally every year, yet the consensus of determining factors remains mixed and obscure. Based on a systematic literature search, I reviewed 184 disaggregate empirical studies of the determinants of childhood malnutrition in Africa published since 1990. The literature concerning disaggregate empirical studies of childhood malnutrition is found wanting for answers to two essential questions: What are the determinants of malnutrition? And how much do the determinants affect malnutrition? The role of spatial heterogeneity, hierarchical institutions, and divergent causal pathways of various non-illness related latent determinants is growing. Few studies consider conflict and environment etiologies: despite being the primary factors attributed to malnutrition, hunger, and death in most catastrophic famine events. Despite an extensive body of research, I find there are a number of opportunities for development within this corpus, in terms of an unmet need for more studies with broad temporal, spatial, and hierarchical perspectives, with an exhaustive set of nutrition outcomes, and findings that are quantifiable and epidemiologically significant.

3.2 Introduction

Universally recognized as a widespread and detrimental condition, malnutrition of young children encompasses a large body of research. Timing of observable determinant factors and measured nutrition outcomes makes identification difficult.

Issues of reverse causality with respect to illness, and highly correlated behavioral determinants make matters worse (Buisman et al., 2019). My review examines the existing status of disaggregate empirical studies of childhood malnutrition, confounding factors, existing policies, current challenges, and future solutions. I sought to analyze the relevant body of research concerning childhood malnutrition determinants over the past 30 years. Africa, being the most food insecure continent, was an obvious place to start (Devereux, 2018). The core identification criteria consisted of empirical and disaggregate child malnutrition studies conducted in African countries since 1990. Specifically, I analyzed the relevant methodologies, locations, outcomes, etiological themes, and conclusions.

3.2.1 Evidence Before this Study

Since the introduction of the UNICEF (1990) theoretical framework there has been an upsurge in studies attempting to corroborate it with empirical evidence. Previous systematic reviews have synthesized various determinants of malnutrition. Bhutta et al. (2008) synthesize the literature of illness related interventions on child malnutrition outcomes. Possible interventions included: balanced energy protein supplementation; vitamin A, zinc, iron, and iodine supplementation; breastfeeding promotion and support; complementary feeding interventions; hygiene interventions; and preventive treatment. They find that if nutrition-specific intervention (including management of acute malnutrition and multiple micronutrient supplementation) is scaled up to 90% coverage, stunting would be reduced by 20% reduce under-five mortality by 15% globally.

Keino et al. (2014) explore the determinants of stunting and overweight across sub-Saharan Africa from 1990 to 2012. Their systematic review yielded a set of 38 studies from the PubMed database. After the screening process, they managed to review 18 studies. With the aid of chi-square tests, they conclude that stunting prevalence rates are dependent on a child's sex, mother's education, mother's occupation, household income, sanitation facilities, and rural living conditions.²

Phalkey et al. (2015) sought to assess the evidence base for climate change impacts on childhood undernutrition (i.e., stunting) in subsistence farming households. Their systematic review across all low- and middle-income countries with no limits on temporal scope and including full-text gray literature documents (from Eldis, Popline, IFPRI, WHOLIS, Agris, AgSpace, and Scirus) along with peer-reviewed studies (from PubMed, Web of Knowledge, OvidSP, EBSCO, and Scopus) yielded 1900 total hits. After the screening process, they reviewed a combined 15 studies. They show much of the evidence for the impact of climate on childhood malnutrition is based on a few heterogeneous studies with flawed methodologies. However, they suggest there are significant but variable linkages between rainfall, temperature, seasonality and extreme weather events with stunting prevalence.

Akombi et al. (2017) reviewed the literature for consistent factors associated with child undernutrition across sub-Saharan Africa. Their systematic review of studies published between January 1990 and January 2017, across 49 sub-Saharan

² Given the low number of included studies from such a large potential population, I suspect the search criteria of returning a false literature pool (i.e., they apply the Boolean operator "AND Africa" unconventionally, which returned only 38 titles in total and disproportionately favored studies from "South Africa" by 53%).

African countries, yielded a set of 2291 individual articles from five bibliographic databases (Scopus, PubMed, PsycINFO, CINAHL and Embase). After the screening process, 49 academic articles were assessed. They report vitamin A and zinc deficiency, low mother's education, increased child's age, male sex of the child, poor households, prolonged duration of breastfeeding, low birth weight, decreased mother's age, unimproved drinking water, low mother's Body Mass Index, small birth size, recent diarrheal episode, low father's education, and rural residences are the consistent determinants of child malnutrition.³

Brown et al. (2020) conduct a structured review of 90 studies that assess relationships between potential determinants and child malnutrition indicators. Their search criteria, with no limits on temporal and geographic scope, yielded a set of 688 articles from the EconLit database. They synthesize the findings of studies with statistically significant positive or negative relationships between child malnutrition and various factors. They identify shocks due to variations in climate conditions (temperature, rainfall, and vegetation indicators) and violent conflict are consistent predictors of child malnutrition. They found factors associated with stunting, wasting, and underweight, including: child's age, multiple births, mother's education, mother's BMI, household's wealth/assets, and national GDP per capita. In addition, child's sex was associated with stunting, and wasting, while rural households and national female education level were associated with stunting, and underweight.⁴

³ Contain various instances of errors, both of commission and omission. For example, exclusion of important articles, inclusion of inappropriate articles, misinterpretation of study designs, miscounting of sample sizes, misattribution of determinizing factors, and mischaracterization of research quality.

⁴ Consists of a limited number of studies given the search criteria. Results of the analysis rest on a misguided understanding of quantitative methods and statistical analysis.

3.2.2 Inflection Points

The year 1990 marks the genesis of the literature with the inception of the UNICEF (1990) theoretical framework. First published in 1993, the Global Burden of Disease Study is an ongoing international collaborative effort to assess the mortality and morbidity of major diseases, injuries, and risk factors including child malnutrition. Each subsequent publication of the Global Burden of Disease Study pertaining to child malnutrition (2008, 2013, and 2018) serve as possible points of activity in the literature. In 1996, the World Food Summit set a target of halving the number of undernourished people globally by 2015. And in 2000, the United Nations adopted the Millennium Development Goals, which included targets to reduce hunger and child mortality by 2015. In 2006 the World Health Organization released the Multicentre Growth Reference, the latest definitive and most ubiquitous international anthropometric child growth chart. As the sun was setting on the Millennium Development Goals, the United Nations adopted the updated and more extensive Sustainable Development Goals in 2015.

From 1991 to 1992, Somalia experienced a famine caused by drought and civil war killing an estimated 300,000 to 500,000 people (Devereux, 2000). In 1998 Sudan experienced a famine caused by drought and civil war killing an estimated 70,000 people (Devereux, 2000). From 1998 to 2000, Ethiopia experienced a famine, worsened by the Eritrean-Ethiopian War, killing an estimated 71,600 to 122,700 people (Salama et al., 2001). From 2001 to 2002, Malawi experienced a famine that killed an estimated 47,000 to 85,000 people (Devereux & Tiba, 2006). From 2004 to 2005, Niger experienced a famine that killed an estimated 13,297 to 47,755 people

(Rubin, 2009). From 2010 to 2012, southern and central Somalia experienced a famine brought on by drought and poor harvests, that killed an estimated 244,000 to 273,000 people (Robinson et al., 2014). Famine conditions have been ongoing in parts of South Sudan from 2017 to the present (IPC, 2017). Each of these multinational endeavors and humanitarian catastrophes serve as potential catalysts to spur activity within the literature.

3.3 Materials and Methods

The core identification criteria consisted of empirical and disaggregate child malnutrition studies conducted in African countries since 1990. I conducted a systematic database search methodology using Web of Science to procure the primary population of potential studies. Web of Science is a comprehensive academic literature database citation index, with coverage across many different academic fields and databases (for explicit Web of Science search concepts with Boolean and Truncation/Wildcard symbology search terms, see section 7.1.1). The Web of Science search yielded a set of 903 articles. I augmented the database search with keyword and citation methodologies using Google Scholar along with hand searching within key journals and articles. Additional identified records from alternative means yielded a set of 73 articles. All duplicate articles were removed to constitute a total of 942 articles for screening.

A further 489 articles were excluded based on their titles, plus an additional 202 articles were excluded based on the content of their abstracts. A total of 251 articles were included in a full-text assessment. Only disaggregate empirical studies of the determinants of childhood malnutrition in Africa published since 1990 were

included in the final selection. All article searches were conducted only in English, and only English-language articles are included in the analysis. Focusing exclusively on anglophone science should be a cause for concern and the practice carries with it greater epistemological issues that are beyond the scope of this paper. However, in a follow-up Web of Science search where English-language only articles is not specified only 11 additional papers are returned, none of which would have been included based on title and abstract screening. In addition, I screened for deceptive or predatory scholarly publishers and journals (see Beall, 2017; Strielkowski, 2017, 2018). During the full-text assessment an additional 67 articles were excluded. The sample included in the final synthesis therefore consists of a total of 184 studies. A flow chart of the article selection process is shown in Figure 2 (Page et al., 2021).

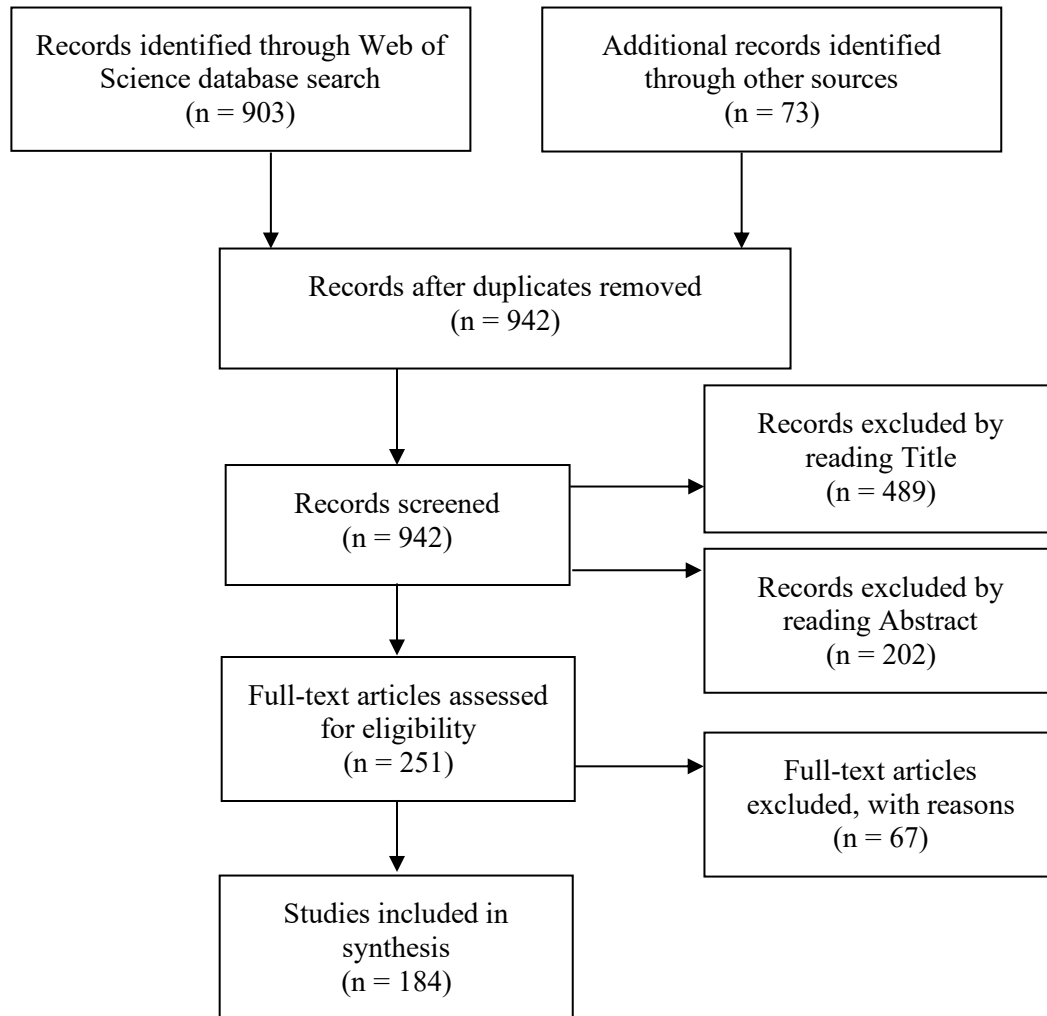


Figure 2: PRISMA systematic review methodology flowchart diagram

During the differentiation process certain themes emerged to guide the screening strategy. Malnutrition and outcome specific exclusion criteria included: studies where malnutrition was used to define a cohort wherein other comorbidities are assessed; studies where malnutrition was strictly coupled with other co-occurrences; studies where malnutrition was used to measure outcome of specific intervention treatment or is an associated risk factor for some other outcome; studies where malnutrition was an independent variable; studies where treatment or prognosis of malnutrition related illnesses or co-morbidities was assessed; studies where

malnutrition prevalence or incidence was not used (e.g., distribution, spatial inequality, composite index of anthropometric failure, stunting shortfall); and studies not assessing stunting, wasting, or underweight related outcomes in the form of alternative measurements, indices, or indicators of nutrition (e.g., the double burden, Mid-Upper Arm Circumference, anemia, environmental hazards, infection, or mortality).

Other methodical exclusion criteria included: studies where a specific or single illness related determinant was tested; studies which occurred outside of Africa; studies of mothers, adults, or children older than (approx.) 59 months, studies that were merely surveys, datasets, or profiles without any empirical or etiological analysis; studies that were strictly program impact evaluations; studies not published in peer-reviewed scholarly journals; studies published in deceptive or potentially predatory scholarly publishers and journals; studies not published between 1990 and 2020; studies not written in English, and all other obviously unrelated or spurious search results. Common exclusion criteria during the full-text article assessment process include: no article access, does not satisfy core inclusion criteria, composite outcome, purely study area description, potentially predatory publisher, overlarge age range, multi-country pooled study, and malnutrition indicator as independent variable.

3.4 Literature Characteristics

I reviewed and analyzed 184 disaggregate empirical studies of the determinants of childhood malnutrition in Africa published since 1990 (Table 1). The collection spans 30 years and 34 countries. Country specific sample sizes range from 93 to 73,778 children. Together 38% of studies (n=70) are nationally representative.

3.5 Childhood Malnutrition Literature in Africa Since 1990

Table 1. Summary of scrutinized studies, grouped alphabetically by country in order of publication year

<i>Study</i>	<i>Outcome</i>	<i>Sample size</i>	<i>Location</i>	<i>Analytical method</i>	<i>Determining factor</i>	<i>QC</i>
(Fernandes et al., 2017)	Stunting Wasting —	742 young children	Bom Jesus, Angola	Bivariate and Poison analysis	Child's age/sex, neighborhood, water source, mother's age, family size/structure	1
(Humbwawali et al., 2019)	Stunting — Underweight	749 children under 2 years	Suburban Cacucaco, Angola	Bivariate and logit analysis	Diarrhea incidence, sibling death, primary caregiver, mother's employment, prenatal care	1
(Padonou et al., 2014)	HAZ WHZ —	520 children 0 to 18 months	Southern Benin	Univariate analysis and multivariate linear mixed model analysis	Birth weight, maternal stature/weight	1
(Tharakan & Suchindran, 1999)	Stunting Wasting Underweight	734 young children	Botswana	Bivariate, logistic, and ordered logistic analysis	Child's age, birth weight, breast-feeding duration, family head, residence, house type, toilet facility, mother's education, father's education, child caretaker, diet, cough/diarrhea incidence	1
(Nnyepi, 2007)	Stunting Wasting —	522 children 0 to 5 years	Gaborone, Botswana	Chi-Square tests, bivariate/multivariate logistic analyses	Child's age/sex, birth weight, adequacy of food, clinic location	2a
(Beiersmann et al., 2013)	— Wasting —	460 young children	Nouna, Burkina Faso	Bivariate and logit analysis	Child's age, religion, presence of younger siblings, village,	1
(Grace et al., 2017)	HAZ — WAZ	1627 children under 25 months	Burkina Faso	Linear regression	Water source cleanliness/reliability, maternal health/nutrition during pregnancy, breastfeeding practices	1
(Poda et al., 2017)	Stunting Wasting Underweight	6337 children under 5 years	Burkina Faso	Univariate analysis and multivariable logistic regression	Child's age/sex, birth size, child morbidity factors, mother's education/body mass index, household wealth	1
(Nkurunziza et al., 2017)	Stunting — —	6199 children 6 to 23 months	Burundi	Bivariate and multivariable logistic regression	Child's sex, birth size, mother's education/assessment, delivery location, family size/wealth	1

(Nagahori et al., 2015)	Stunting Wasting Underweight	100 children under 2 years	Urban Batouri, Cameroon	Wilcoxon rank sum test, Fisher's exact test	Child's age, mother's age/education, family planning information, water source	1
(Nagahori et al., 2017)	Stunting Wasting Underweight	212 children 0 to 5 years	Batouri, Cameroon	Multiple linear regression analysis	Complementary feeding	1
(Begin et al., 1997)	HAZ — —	93 children 12 to 71 months	Rural Chad	Linear regression	Child's age, mother's feeding autonomy, father's cereal sales	2b
(Emina et al., 2011)	Stunting Wasting —	9748 children under 5 years	D.R.C.	Logit generalized estimating equation	Mother's education, providence	2b
(Kandala et al., 2011)	HAZ — —	3663 children under 5 years	D.R.C.	Markov chain Monte Carlo geo-additive semi- parametric mixed model	Child's age/sex, mother's education/body mass index, household wealth, residence, province	1
(Kismul et al., 2018)	Stunting — —	9030 children under 5 years	D.R.C.	Bivariate and Multivariate logistic regression	Child's age/sex, mother's age/height/ education/body mass index, breastfeeding practice, water access, hygienic toilet access, number of children in family, household wealth, residence, province	1
(McKenna et al., 2019)	Stunting Wasting —	3721 children 6 to 59 months	D.R.C.	Chi-square tests, Bivariate and Multivariate Logistic regression	Child's age/sex, household socioeconomic status, mother's age/education, number of under five children in household, residence, province	1
(Delpeuch et al., 2000)	HAZ WHZ WAZ	1163 young children	Urban Brazzaville, Congo	General linear and logistic analysis	Economic level of the household, mother's education, dwelling location	1
(Kavle et al., 2015)	Stunting — —	7794 (2005) and 6091 (2008) children 6 to 59 months	Egypt	Bivariate analyses, Pearson's chi-square, and multivariable logistic regression	Dietary diversity, poultry consumption, sugary foods consumption	2a
(Kavle et al., 2016)	HAZ WHZ WAZ	277 longitudinal cohort 0 to 1 year at 1-year interval	Egypt	Bivariate linear and logistic regression, multivariate mixed models	Minimum dietary diversity, diarrhea/fever incidence, program exposure	1
(Rashad & Sharaf, 2018)	Stunting Wasting Underweight	43,446 to 40,712 children under 5 years	Egypt	Logistic regression with regional/time fixed effects	Child's age/sex, twin birth, birth interval, maternal healthcare/occupation, father's education, household size, toilet facilities, economic growth	1
(Lindtjørn & Alemu, 2002)	Stunting — —	678 young children	Rural Ethiopia	Correlation, Student's t-test, Yates chi-square test, and survival analysis	Child's age/sex	1

(Teshome et al., 2009)	Stunting — —	622 children 0 to 59 months	West Gojam, Ethiopia	Bivariate and multivariate analysis	Child's age/sex, diarrhea incidence, deprivation of colostrum, duration of breastfeeding, pre-lacteal feeds, type of food, complementary feeding timing, feeding method	2a
(Deribew et al., 2010)	Stunting Wasting Underweight	2410 young children	Gilgel Gibe, Ethiopia	Bivariate and logistic regression	Child's age	1
(Medhin et al., 2010)	Stunting — Underweight	873 to 926 longitudinal cohort 0 to 12 months at 6-month interval	Butajira, Ethiopia	Linear and logistic multiple regression	Child's sex, birth weight, maternal nutritional status, household sanitary facilities, residence	1
(Mulugeta et al., 2010)	HAZ WHZ WAZ	318 young children	Tigray, Ethiopia	Bivariate and regression analysis	Child's age, mother's health, complementary food adequacy, pre-lacteal foods, residential zone	1
(Ali et al., 2013)	Stunting Wasting Underweight	2356 children 6 to 59 months	Ethiopia	Bivariate and multivariate logistic regression	Food insecurity	2b
(Egata et al., 2013)	— Wasting —	2132 children 6 to 36 months	Kersa, Ethiopia	Conditional fixed- effects logistic regression	Household poverty, access to health services	1
(Egata et al., 2014)	— Wasting —	2199 children 6 to 36 months	Kersa, Ethiopia	Bivariate and multivariable logistic regression	Household poverty, access to health services, decision making power, birth interval, breastfeeding practice	1
(Fikadu et al., 2014)	Stunting — —	242 children 24 to 59 months	Meskan, Ethiopia	Bivariate analysis, backward stepwise logistic regression	Family size, number of under-five children in the household, maternal occupation, duration of exclusive breastfeeding, duration breast feeding, complementary food feeding method	1
(Alemayehu et al., 2015)	Stunting Wasting Underweight	605 young children	Tigray, Ethiopia	Bivariate and logit analysis	Child's age/sex, breast feeding practice, mother's education, father's education, family food distribution, water source, family size, sanitation facilities, weaning practices, family financial distribution	1
(Asfaw et al., 2015)	Stunting Wasting Underweight	796 young children	Bule Hora, Ethiopia	Bivariate and logit analysis	Child's sex, diarrhea incidence, pre-lacteal feeding, complementary feeding timing, contraception use	2b
(Fekadu et al., 2015)	Stunting Wasting Underweight	214 children under 2 years	Filtu, Ethiopia	Bivariate and logit analysis	Breastfeeding, diarrhea incidence, diet diversity, complementary feeding, bottle feeding	2b
(Motbainor et al., 2015)	Stunting Wasting Underweight	3964 young children	Amhara, Ethiopia	Linear regression	Food insecurity, food diversity, number of meals, residence	1

(Yisak et al., 2015)	Stunting Wasting Underweight	791 young children	Haramaya, Ethiopia	Bivariate and logit analysis	Child's sex, birth order, family size, diarrhea/fever incidence, mother's body mass index, antenatal care, pre-lacteal feeding, residence	2b
(Fentahun et al., 2016) [‡]	Stunting Wasting Underweight	1927 children under 5 years	Oromiya, Ethiopia	Two-level mixed-effects logistic regression model	Child's age/sex, siblings, diet diversity, feeding of special foods during illness	2c
(Haile et al., 2016) [‡]	Stunting — —	9893 children 0 to 59 months	Ethiopia	Multilevel logistic regression (2-level)	Child's age/sex, birth interval, severe anemia, mother's education/body mass index, father's education, head of household gender, household wealth, improved latrine facility availability	1
(Tariku et al., 2016)	Stunting — —	681 children 24 to 59 months	Dembia, Ethiopia	Bivariable and multivariable binary logistic regression	Latrine availability, household size	1
(Alemu et al., 2017) [‡]	HAZ — —	3108 young children	East Gojjam, Ethiopia	Multilevel linear regression analysis	Child's age/sex, immunization status, diarrheal morbidity, breast feeding, mother's nutritional status, number of under-five children in the household, household water treatment, household dietary diversity, agroecosystem type, liquid waste disposal, latrine utilization	2b
(Batiro et al., 2017)	Stunting — —	465 children 6 to 59 months	Kindo Didaye, Ethiopia	Bivariate analysis, multivariate logistic regression	Vaccination status, drinking water source, animal source food, acute respiratory infection incidence, breastfeeding initiation	2b
(Betebo et al., 2017)	Stunting Wasting Underweight	508 children 6 to 59 months	East Badawacho, Ethiopia	Bivariate analysis, multivariate logistic regression	Child's age/sex, birth interval, diarrhea incidence, pre-lacteal feeding, complementary feeding initiation, mother's health during pregnancy, antenatal care visits, household food insecurity	2a
(Darsene et al., 2017)	Stunting Wasting Underweight	811 children 6 to 59 months	Hawassa, Ethiopia	Bivariate logistic regression analysis	Child's sex, diarrheal morbidity, birth interval, mother's age/education, colostrum feeding, breastfeeding cessation timing, complementary feeding frequency, family size	2a
(Demilew & Abie, 2017)	Stunting — Underweight	480 children 2 years old	Bahir Dar slums, Ethiopia	Bivariate and logit analysis	Illness incidence, pre-lacteal feeding, complementary feeding initiation timing, number of under-three children in the household, latrine utilization, hand washing practices	2a
(Tariku, Bikis, et al., 2017)	— Wasting —	1184 children 6 to 59 months	Dabat, Ethiopia	Binary and multivariate logistic regression	Dietary diversity, breastfeeding initiation, postnatal vitamin-A supplementation, mother's occupation	1
(Tariku, Biks, et al., 2017)	Stunting — —	1295 children 6 to 59 months	Dabat, Ethiopia	Bivariable analysis, ordinal multivariable logistic regression	Mother's occupation, postnatal vitamin-A supplementation, household wealth, family food from farms	1
(Woodruff et al., 2017)	HAZ — —	23,999 children under 5 years	Ethiopia	Bivariate linear regression and ANOVA, pooled multivariate linear regression	Diarrhea/fever incidence, mother's height/education/nutrition status, contraception usage, toilet facility, community location	1

(Wubante, 2017)	Stunting Wasting Underweight	400 children under 1 year	Dabat, Ethiopia	Bi-variate analysis, multiple logistic regression	Deprivation of colostrum, mother's age, radio ownership, toilet facility, complementary feeding method	2a
(Abeway et al., 2018)	Stunting — —	410 children 6 to 59 months	Merhabete, Ethiopia	Binary and multivariable logistic regression	Child's age/sex, birth weight, complementary food initiation, mother's education, antenatal care	2a
(Ahmadi et al., 2018)	Stunting Wasting Underweight	1005 children under 5 years	Ethiopia	ANOVA, t-test, and linear regression	Child's age/sex, mother's education/mid-upper arm circumference, open defecation	1
(Berhanu et al., 2018)	Stunting — —	1039 children 24 to 59 months	Albuko, Ethiopia	Bivariable and multivariable logistic regression	Child's sex, birth order, dietary diversity score, mother's education/nutrition status, family size, food insecurity, water access	2a
(Geberselassie et al., 2018)	Stunting — —	1287 children 6 to 59 months	Libo-Kemekem, Ethiopia	Bivariate and multivariable logistic regression	Child's age, family size, father's education, household head occupation, parental employment	2a
(Gelu et al., 2018) [‡]	Stunting Wasting —	593 young children	Gondar slums, Ethiopia	Bivariate and logit analysis	Child's age, fever incidence, wealth status, parental financial control	2b
(Nigatu et al., 2018)	— — Underweight	645 children 6 to 59 months	Takusa, Ethiopia	Bivariate and multivariable logistic regression	Antenatal care, mother's age, residence	2a
(Wasihun et al., 2018)	Stunting Wasting Underweight	610 children 6 to 59 months	Tigray, Ethiopia	Bivariate and multivariable logistic regression	Child's age, hand washing, family size	2a
(Amare et al., 2019)	Stunting Wasting —	9419 children 0 to 59 months	Ethiopia	Bivariate and logistic regression	Child's age/sex, birth weight, mother's education/stature/body mass index, household wealth, toilet facility type, cooking fuel type, residence, region	1
(Berhe et al., 2019)	Stunting — —	330 children 6 to 24 months	Mekelle, Ethiopia	Bivariate and multivariate logistic regression	Birth weight, diet diversity score, diarrhea incidence, mother's education/height/body mass index, household number of children	1
(Dake et al., 2019)	Stunting — —	342 children 6 to 59 months	Sodo Zuria, Ethiopia	Bivariate and multivariate logistic regression	Child age/sex, pre-lacteal feeding, diarrhea incidence, family planning, income	1
(Dessie et al., 2019)	Stunting Wasting —	7452 children 6 to 59 months	Ethiopia	Binary logistic regression and multivariable analysis	Birth interval, mother's education/anemia/nutrition status, place of delivery	1
(Gebre et al., 2019)	Stunting Wasting Underweight	840 children 6 to 59 months	Afar, Ethiopia	Bivariate and multivariable logistic regression	Child's age/sex, immunization status, pre-lacteal feeding, diarrhea incidence, mother's education, family size	2a

(K. F. Gebru et al., 2019) [‡]	Stunting — —	8855 children under 5 years	Ethiopia	Bivariate and multilevel logistic regression	Child's age/sex, birth size, twin status, mother's education, household wealth, religion, community	2a
(T. T. Gebru et al., 2019)	— Wasting —	394 children under 5 years	Wukro, Ethiopia	Bivariate and multivariable logistic regression	Family cohesion, family planning	2a
(Kwami et al., 2019)	HAZ — —	2400 children under 5 years	Amhara, Oromiya, SNNPR, and Tigray, Ethiopia	Bivariate and multivariate linear regression	Child's age, caregiver gender, drinking water source, handwashing after defecation, handwashing before eating	2a
(Mohammed et al., 2019)	HAZ — —	2932 children 6 to 23 months	Ethiopia	Bivariable and multivariable linear regression	Child's age/sex, birth size, meal frequency, dietary diversity score, breastfeeding status, vitamin A supplementation, household wealth, household toilet facility, region	1
(Motbainor & Taye, 2019)	— Wasting —	862 children 6 to 59 months	Libokemkem, Ethiopia	Binary and multivariate logistic regression	Diarrhea incidence, complementary feeding practice, mother's empowerment/education, household income, non-rice producing communities	1
(Nigatu et al., 2019)	Stunting Wasting Underweight	2433 children under 6 months	Ethiopia	Bi-variable logistic regression	Exclusive breastfeeding timing	2a
(Takele et al., 2019) [‡]	Stunting — —	8743 children under 5 years	Ethiopia	Generalized linear mixed model (GLMM)	Child's age/sex, birth interval, breastfeeding period, mother's education/body mass index, household wealth, toilet facility type, drinking water source, internet use	2b
(Nabwera et al., 2018)	— Wasting —	280 children 6 to 59 months	West Kiang, Gambia	Univariable analysis and conditional logistic regression	Complementary feeding frequency	2a
(Rikimaru et al., 1998)	— — WAZ	170 children 8 to 36 months	Accra, Ghana	Pearson's chi-square test, Tukey's test, pair-wise correlation	Birth weight, feeding frequency, breast-feeding access, co-parental support, mother's age/education/occupation, father's education/occupation,	1
(Ruel et al., 1999)	HAZ — —	475 young children	Accra, Ghana	Ordinary least squares and instrumental variable two-stage least squares	Child's age, mother's height/education, care practices, housing quality, household assets	2c
(Nikoi & Anthamatten, 2013) [‡]	HAZ — —	2225 young children	Ghana	Multilevel analysis	Child's age, birth size, vaccination status, breast-feeding duration, mother's body mass index, health insurance access, household wealth, population density	1
(Darteh et al., 2014)	Stunting — —	2379 young children	Ghana	Logit analysis	Child's age, number of siblings, mother's age, region	2b

(Aheto et al., 2015) [‡]	HAZ WHZ WAZ	2083 young children	Ghana	Multilevel analysis	Child's age, birth size, twin status, breast-feeding duration, diarrhea incidence, mother's education/body mass index, toilet facility access household income, national health insurance access	1
(Wemakor & Mensah, 2016)	Stunting — —	384 children 0 to 59 months	Northern Ghana	Chi square tests and multivariate logit regression	Mother's depression	2a
(Saaka & Galaa, 2016)	Stunting Wasting —	2720 young children	Ghana	Bivariate and logistic analysis	Child's age, birth weight, prenatal care, mother's height, household wealth, residence,	1
(Atsu et al., 2017)	Stunting — —	7750 young children	Ghana	Bivariate and Poisson analysis	Mother's age, household wealth, religion	2b
(Aheto et al., 2017)	HAZ — —	10,036 children under 5 years	Ghana	Dynamic linear state-space model with backwards selection	Child's age, breastfeeding duration, mother's years of education	1
(Ewusie et al., 2017)	Stunting Wasting Underweight	2379 children under 5 years	Ghana	Univariate analysis and multivariate logistic regression	Child's age/sex, mother's education/nutritional status of the mother, household financial status	1
(Bandoh et al., 2018)	Stunting Wasting Underweight	250 children 6 to 59 months	Ekumfi Narkwa, Ghana	Simple logistic regression	Caregiver age	2b
(Nikoi, 2018) [‡]	— — Underweight	2244 children 0 to 59 months	Ghana	Generalized linear mixed models	Child's sex, birth size, fever incidence, mother's body mass index, insurance coverage, number of under five children in household, culture, geography	2b
(Boah et al., 2019)	Stunting Wasting Underweight	2720 children 0 to 59 months	Ghana	Single multiple logistic regressions	Child's age/sex, birth weight, minimum diet diversity, birth order, paternal education, mother's autonomy/body mass index, household wealth, region	2a
(Woodruff et al., 2018)	Stunting Wasting —	9228 children under 60 months	Guinea	Bivariate analysis and logistic regression	Birth size, child health/nutritional status, child caring practice, mother's nutritional/health status, household water source, sanitation facilities	1
(Thorne et al., 2013)	Stunting — —	872 children 0 to 59 months	Bijagós, Guinea-Bissau	Univariate analysis and logistic regression	Immunization status, parent's education, size of living quarters, water source, feeding practices	1
(Onyango et al., 1998)	HAZ WHZ WAZ	154 children 12 to 36 months	Rural western Kenya	Forward selection backward elimination linear regression	dietary diversity, starchy gruel complementation	1

(Bloss et al., 2004)	Stunting Wasting Underweight	175 young children	Ugunja, Kenya	Bivariate and logit analysis	Child's age, vaccination status, weaning practices, adoption status	1
(Kabubo-Mariara et al., 2009)	HAZ/Stunting — —	5870 young children	Kenya	Multiple regression	Child's sex, mother's education, birth count, contraceptive usage, household assets, public health services	2b
(Abuya et al., 2011)	Stunting — —	5949 young children	Kenya	Multivariate logistic regression	Mother's education	2b
(Olack et al., 2011)	Stunting Wasting Underweight	1245 children 6 to 59 months	Nairobi informal settlements, Kenya	Chi-square test	Child's age/sex	1
(Abuya et al., 2012)	Stunting — —	4770 young children	Nairobi slums, Kenya	Bivariate and logit analysis	Child's sex, birth weight, mother's education, marital status, parity, health seeking behavior, social economic status	2b
(Gewa & Yandell, 2012)	Stunting Wasting Underweight	3793 young children	Kenya	Bivariate and logit analysis	Child's sex, birth size, diarrhea/cough incidence, immunization status, breast-feeding duration, mother's education/body mass index/age at first birth, household wealth, residence, season	2b
(Grace et al., 2012) [‡]	Stunting — —	2255 young children	Kenya	Multilevel analysis	Mother's education, source of drinking water, household wealth, livelihood zone, precipitation level	2b
(Fotso et al., 2012) [‡]	Stunting — —	Up to 3693 children across 6 cohorts and 8 surveys	Nairobi informal settlements, Kenya	Univariate, bivariate, and multivariate models with random intercept multilevel regression	Child's age/sex, mother's education, marital status, food access, assets, residence	2c
(Faye et al., 2019)	HAZ — —	1917 children under 5 years	Nairobi informal settlements, Kenya	Generalized linear model	Child's age, birth weight, immunization status, breast-feeding practice, mother's age, marital status, socio-economic status, household size	1
(El Taguri et al., 2009)	Stunting — —	4549 children under 5 years	Libya	Bivariate and multivariate logit analysis	Child's age/sex, birth weight, diarrhea incidence, psychosocial stimulation, father's age/education, water access/storage, garbage disposal, residence	1
(Rabaoarisoa et al., 2017)	Stunting — —	1826 children 6 to 59.9 months	Moramanga and Morondava, Madagascar	Backwards stepwise multivariate logistic regression	Child's age, birth size, infection incidence, birth interval, mother's activities, household income	2a
(Rakotomanana et al., 2017)	Stunting — —	3920 young children	Madagascar	Bivariate analysis and logistic regressions	Child's age/sex, mother's height, iodized salt use, residence	1

(McCuskee et al., 2018)	Stunting Wasting —	1175 children 6 to 59 months	Ifanadiana, Madagascar	Univariate and multivariate logistic regression	Child's age, birth size, mother's weight/height/body mass index, father's height	2a
(Espo et al., 2002)	Stunting — —	613 longitudinal cohort 3 to 12 months at 3-month intervals	Malawi	Chi-square tests, univariate analysis and stepwise multivariate logistic regression	Child's sex, birth weight, morbidity in infancy, birth gestation, gestational weight gain, mother's height, weaning practices, socioeconomic status	1
(Maleta et al., 2003)	Stunting Wasting Underweight	767 children 0 to 36 months	Lungwena, Malawi	Univariate analysis and multivariate logit	Birth weight, illness episodes in infancy, mother's HIV status, health facility distance	1
(Kalanda et al., 2005)	— — WAZ	322 longitudinal cohort 0 to 52 weeks at 4-week intervals	Chikwawa, Malawi	Univariate analysis and multivariate logistic regression	Child's sex, birth weight/season, placental or peripheral malaria at delivery, infant illness incidence, mother's height/literacy	1
(Chirwa & Ngalawa, 2008)	HAZ WHZ WAZ	5218, 4370 and 4270 children under 5 years	Malawi	2SLS regressions	Child's age/sex, drinking water access, economic empowerment	1
(Aiga et al., 2009)	— — Underweight	132 young children	Zomba, Malawi	Bivariate and logistic regression	Breastfeeding duration, proportion oil/fat intake, fish farming income	1
(Weisz et al., 2011)	HAZ — WAZ	209 children 6 to 18 months followed >280 days	Rural Malawi	Linear mixed model analysis	Diarrhea/fever/cough duration	1
(Chikhungu & Madise, 2014)	Stunting — Underweight	6687 children 6 to 59 months	Malawi	Chi-square tests and multivariate logit	Child's sex, illness incidence, housing quality, household food expenditure, season	2b
(Chikhungu et al., 2014) [‡]	Stunting — —	4284 children 6 to 59 months	Malawi	Chi-square tests and multilevel logistic regression (two-level random intercept model)	Child's age/sex, food expenditure, daily market/lineage availability, improved floor, permanent roof	2a
(Kuchenbecker et al., 2015)	Stunting Wasting Underweight	196 children 0 to 6 months	Central and northern Malawi	ANOVA	Exclusive breastfeeding	1
(Ntenda & Chuang, 2018)	Stunting Wasting Underweight	6384 children under 5 years	Malawi	Three-level logistic models with generalized estimating equations	Child's sex, birth size, year of birth, diarrhea incidence, twin status, mother's weight/education/socio-economic status, community wealth/female education	2b
(Ntenda, 2019)	Stunting Wasting Underweight	4047 children under 5 years	Malawi	Multivariate logistic regression	Birth weight	2c

(Bouvier et al., 1995)	Stunting Wasting —	491 longitudinal cohort over 5-year period	Sikasso, Mali	Univariate pooled and age stratified logistic regression	Mother's education, father's education, family assets	1
(Hatloy et al., 2000)	HAZ WHZ WAZ	2315 children 6 to 59 months	Koutiala, Mali	Pearson's chi-square test, Student's test, and logistic regression	Food variety score, diet diversity score, region	2b
(Grace et al., 2016) [‡]	Stunting — —	2830 young children	Mali	Standard generalized linear models and generalized linear mixed models (multilevel)	Child's sex, birth year, mother's height, father's education, household wealth, region	2b
(García Cruz et al., 2017)	Stunting — —	282 children under 5 years	Tete, Mozambique	T-test, ANOVA, bivariate analyses, chi-square test, stepwise multiple logistic regression	Birth weight, breastfeeding duration, complementary feeding timing, mother's education/occupation, number of under-five children in the household, family size, charcoal use, housing infrastructure, region	1
(Ighogboja, 1992)	Marasmus Kwashiorkor Marasmic Kwashiorkor	900 young children	Jos, Nigeria	Pearson's chi-squared test	Mother's education, weaning practices, household income	1
(Abidoye & Ihebuzor, 2001)	Stunting Wasting Underweight	365 young children	Lagos slums, Nigeria	Pearson's chi-squared test	Food/feeding practices, immunization status, parent's education, living quarters size, water source	1
(Esimai et al., 2001)	Stunting Wasting —	344 young children	Ilare, Nigeria	Pearson's chi-squared test	Child's sex, family socioeconomic situation	1
(Ojofeitimi et al., 2003)	Stunting Wasting —	230 young children	Oranfe, Nigeria	Pearson's chi-squared test	Child's age, immunization status, mother's age/education/parity, family type	1
(Ukwuani & Suchindran, 2003)	Stunting Wasting —	5331 young children	Nigeria	Pearson's chi-squared test and ordinal logistic regression	Diarrhea incidence, breast-feeding duration, accompanying mother to work, mother's occupation	2b
(Odunayo & Oyewole, 2006)	Stunting Wasting —	420 young children	Ifewara, Nigeria	Pearson's chi-squared test	Child's age, feeding practices, infant formula use, mother's income, parental education, standard of living, overcrowding,	1
(Uthman, 2008) [‡]	Stunting — —	4007 young children	Rural Nigeria	Multilevel logit analysis	Mother's weight, maternal health-seeking behavior, duration of breastfeeding, household wealth, heterogeneity across individual/community levels	2b
(Ajao et al., 2010)	Stunting Wasting Underweight	412 young children	Ife, Nigeria	Logit analysis	Mother's education/finances	1

(Olusanya et al., 2010)	Stunting Wasting —	5888 young children	Lagos, Nigeria	Bivariate analysis and logit regression	Child's sex, antenatal care, place of delivery, hyperbilirubinemia, mother's age/education/parity, multiple pregnancies, residence	1
(Olusanya & Renner, 2012)	Stunting Wasting Underweight	2754 young children	Lagos, Nigeria	Conditional logistic regression	Place of delivery	2a
(Adekanmbi et al., 2013) [‡]	Stunting — —	28,647 young children	Nigeria	Multilevel logit analysis	Child's age/sex, birth weight, twin status, birth interval, mother's education/body mass index, maternal health-seeking behavior, household wealth, community literacy rates, region	2b
(Idris et al., 2013)	Stunting Wasting Underweight	119 young children	Biye, Nigeria	Pearson's chi-squared test	Family size, feeding practices	1
(Senbanjo et al., 2013)	Stunting Wasting Underweight	150 young children	Alimosho and Epe, Nigeria	Pearson's chi-squared test	Child's age/sex, birth order, father's education, social class	1
(Balogun & Yakubu, 2015)	Stunting Wasting Underweight	366 young children	Shika, Nigeria	Logistic regression	Diarrhea incidence, father's education	1
(Ogunlesi et al., 2015)	— Wasting —	208 young children	Sagamu, Nigeria	Cross-sectional analysis	Infection, mother's education, breastfeeding practices/timing, weaning timing	1
(Udoh & Amodu, 2016)	Stunting Wasting Underweight	330 children 6 to 11 months	Akpabuyo, Nigeria	Bivariate Chi square tests, Multivariate logistic regression	Complementary food intake, dietary diversity, feeding frequency	1
(Blessing J. Akombi, Kingsley E. Agho, Dafna Merom, et al., 2017) [‡]	— Wasting Underweight	24,529 children 6 to 59 months	Nigeria	Multilevel analysis	Child's sex, birth size, mode of delivery, fever incidence, mother's body mass index, geopolitical zone	1
(Blessing J. Akombi, Kingsley E. Agho, John J. Hall, et al., 2017) [‡]	Stunting — —	24,529 children 6 to 59 months	Nigeria	Multilevel analysis	Child's sex, birth size, diarrhea incidence, breastfeeding duration, mother's body mass index, household wealth, geopolitical zone	1
(Amare et al., 2018)	Stunting — —	4495, 4183, and 3601 children 6 to 23 months	Northern Nigeria	Maximum-likelihood logit regression	Child's sex/age, birth order, diet diversity, vitamin A supplements, birth facility, mother's body mass index, antenatal clinic, radio/television use, household wealth	2c
(Agu et al., 2019)	Stunting Wasting Underweight	7532 children 3 to 24 months	Nigeria	Weighted bivariate and multi-variable logistic regression	Breast-feeding practice, mother's education/body mass index, marriage type domestic violence incidence, ethnicity, socio-economic status	2b
(Gayawan et al., 2019)	Stunting Wasting Underweight	24,505 children under 5 years	Nigeria	Bayesian quantile regression	Child's sex, birth order, diarrhea/fever incidence, breastfeeding practice, vitamin A supplements, mother's education, household wealth, toilet facilities, newspaper/tv use, residence	1

(Jude et al., 2019)	Stunting Wasting —	749 children 12 to 59 months	Enugu, Nigeria	Chi square tests	Mother's education, socio-economic status	1
(Habimana & Biracyaza, 2019)	Stunting — —	1905 children 6 to 59 months	Rwanda	Univariate and multivariate logistic regression	Child's sex, fortified food intake, breastfeeding practice, antenatal care visits, mother's age/education/occupation, household wealth, toilet facilities	1
(Nshimiyiryo et al., 2019)	Stunting — —	3594 children under 5 years	Rwanda	Logistic regression	Child's sex/age, birth weight, deworming incidence, mother's height/education/literacy, household wealth	1
(Weatherspoon et al., 2019)	Stunting — —	770 children 4 to 25 months	Rural Rwanda	Clustered variance-covariance logit analysis	Child's age/sex/weight, dietary diversity, household head marriage status/education level, mother's height, livestock/family garden presence, altitude, soil fertility, distance to main market road, food production policies	2b
(Simondon et al., 2001)	HAZ WHZ WAZ	436 longitudinal cohort 1.5 to 4 years at 6-month intervals	Senegal	Two-factor ANOVA and multiple linear regression	Weaning age, breast-feeding incidence	1
(Gupta et al., 2007)	Stunting Wasting —	374 children 6 to 23 months	Senegal	Chi-square tests, linear regression, and multiple logistic regression	Child's age/sex, drinking water source, family size, community	1
(Kinyoki et al., 2015) [‡]	Stunting Wasting Underweight	73,778 children 6 to 59 months	Somalia	Bayesian hierarchical spatio-temporal regression analysis	Child's age/sex, fever/diarrhea incidence, household size, food access, conflict events	3
(Kinyoki, Kandala, et al., 2016)	Stunting Wasting Underweight	73,778 children 6 to 59 months	Somalia	Stochastic partial differential equations	Child's age/sex, illness incidence, high protein foods access, carbohydrate access, vegetation cover, temperature	1
(Kinyoki et al., 2017) [‡]	Stunting Wasting Underweight	73,778 children 6 to 59 months	Somalia	Bayesian hierarchical spatio-temporal regression analysis	Child's age/sex, fever/diarrhea incidence, household size, food access, conflict	3
(Dannhauser et al., 2000)	HAZ WHZ WAZ	348 children under 72 months	Bloemfontein, South Africa	Contingency tables	Median nutrient intake together with household income	1
(Chopra, 2003)	Stunting Wasting —	868 young children	Hlabisa, South Africa	Logit regression	Birth weight, breastfeeding practices, mother's education/literacy, father's presence, household construction, toilet facilities	1
(Mamabolo et al., 2005)	Stunting — —	162 children 3 years old	Central Limpopo, South Africa	Binary logistic regression	Mother's occupation, household size	2b

(Theron et al., 2007)	Stunting — —	132 children 12 to 24 months	Rural Limpopo, South Africa	Two-sided t-tests	(Null)	1
(Willey et al., 2009)	Stunting — —	621 children under 3 years	Johannesburg and Soweto, South Africa	Bivariate and logit analysis	Child's sex, birth weight, other's employment, father's education, domestic help employment	1
(Lesiapeto et al., 2010)	Stunting — Underweight	2485 young children	Rural Kwazulu-Natal and Eastern Cape, South Africa	Logistic regression	Child's sex, growth perception, food handouts, breast feeding practices, mother's empowerment/education	1
(Kimani-Murage et al., 2011)	HAZ/Stunting WHZ/Wasting WAZ/Weight	671 children 12 to 59 months	Agincourt, South Africa	Univariate, multivariate linear and logit regression	Child's age, birth weight, household head age, mother's age/HIV status, residence	2a
(Matsungu et al., 2017)	Stunting — —	750 children 6 months old	Matlosana, South Africa	Univariate logistic and multivariable binary logistic regression	Child's sex, birth weight, mother's height, plasma concentrations	2a
(Slemming et al., 2017)	Stunting — —	1098 children 2 years old	Soweto, South African	Bivariate analyses and multiple logistic regression	Birth weight, mother's education, household socio-economic status	2a
(Casale et al., 2018)	HAZ — —	691 children 2 years old	Soweto-Johannesburg, South Africa	Ordinary least-squares regression and probit models	Birth weight, vaccination status, ear/eye illness symptoms, care environment, mother's education	1
(Madiba et al., 2019)	Stunting Wasting Underweight	1254 children 12 to 60 months	Gauteng, South Africa	Binary and multivariate logistic regression	Child's age/sex, birth weight, preschool attendance	1
(Sedgh et al., 2000)	Stunting — —	8174 children 6 to 72 months	Sudan	Univariate and multivariate logistic regression	Child's age/sex, breast-feeding status, carotenoid intake, mother's literacy, household water supply	2c
(Nyaruhucha et al., 2006)	— — Underweight	250 young children	Simanjiro, Tanzania	Summary characteristics	Child's age, breastfeeding/weaning practices, food availability, mother's education, household size	1
(Abubakar et al., 2012)	Stunting — Underweight	423 children under 3 years	Same, Tanzania	Bivariate and logit analysis	Child's age/growth, mother's education, distance to water source	1
(Mamiro et al., 2005)	Stunting — —	309 children 6 months old	Kilosa, Tanzania	Logit analysis	Birth weight, mother's body mass index	2b

(Chirande et al., 2015)	Stunting — —	7324 young children	Tanzania	Bivariate and logit analysis	Child's sex, birth size, mother's education, drinking water source	1
(Semali et al., 2015)	Stunting — —	678 young children	Kongwa, Tanzania	Bivariate and logit analysis	Household head's age/sex, mother's age/education, mobile phone ownership	1
(Nordang et al., 2015)	Stunting — Underweight	152 young children	Rural Rukwa, Tanzania	Bivariate and logit analysis	Illness incidence, mother's farming time, food shortage, dry-season cultivation	1
(Mbwana et al., 2017)	Stunting — —	120 children 6 to 59 months	Morogoro and Dodoma, Tanzania	Logistic regression multivariate analysis	Child's age/sex, duration of breastfeeding, iodized salt use, mother's literacy/body mass index, household size, cultivated land size, distance to water source	2b
(Mgongo et al., 2017)	Stunting Wasting Underweight	1870 children 0 to 24 months	Kilimanjaro, Tanzania	Chi-square tests, Univariate logistic regression, multivariate logistic regression	Child age/sex, birth weight, illness incidence, breastfeeding incidence, mother's education, father's age, district	1
(Kejo et al., 2018)	Stunting Wasting Underweight	436 children 6 to 59 months	Arusha, Tanzania	Bivariate analysis and multivariable binary logistic regression	Child's age/sex, nonexclusive breastfeeding incidence, mother's age, region	1
(Mshida et al., 2018)	Stunting — Underweight	310 children under 5 years	Arusha, Tanzania	Logistics regression	Child's sex, diarrhea incidence, complementary feeding practice, mother's education, family polygamy, surface water use, un-boiled cow's milk consumption	2a
(Muhimbula et al., 2019)	HAZ WHZ WAZ	220 children under 5 years	Morogoro and Shinyanga, Tanzania	Chi-squared tests, multiple linear regression and binary logistic regression	Breastfeeding timing, fluid's introduction, mother's age/height, seasonality	1
(Shilugu & Sunguya, 2019)	Stunting — —	358 children under 5 years	Bukombe, Tanzania	Bivariate and multivariate logistic regression	Child's age, birth weight, feeding practice, dietary diversity, food insecurity, peasant households	1
(Sunguya et al., 2019)	Stunting — —	8815 young children	Tanzania	Logistic regression	Child's age/sex, birth weight, breastfeeding practices, mother's education/body mass index, household wealth, residence	2a
(Kikafunda et al., 1998)	Stunting — Underweight	261 children under 3 years	Central Uganda	Bivariate and logit analysis	Child's age, breastfeeding duration, meal size, food energy density, milk consumption, eye pathology presence, health quality, mother's education, unprotected water use, charcoal/paraffin fuel use, personal hygiene quality, socio-economic status, residence	1
(Wamani et al., 2004)	Stunting Wasting Underweight	721 children under 2 years	Hoima, Uganda	Bivariate and backward conditional logistic regression	Child's sex, fever/cough incidence, deworming incidence, mother's education, father's education, latrine facilities, household wealth	1

(Wamani et al., 2006)	Stunting Wasting Underweight	721 children under 2 years	Hoima, Uganda	Bivariate and backward conditional logistic regression	Child's sex, fever/cough incidence, deworming incidence, mother's education, father's education, latrine facilities, household wealth	1
(Engebretsen et al., 2008)	Stunting Wasting —	723 children under 1 year	Eastern Uganda	Bivariate and logit analysis	Child's age/sex, diarrhea incidence, sibling count, feeding practices, household wealth	1
(Habaasa, 2015)	Stunting Wasting Underweight	104 young children	Nakaseke and Nakasongola, Uganda	Bivariate and logit analysis	Child's age, mother's occupation	1
(Vella et al., 1992)	HAZ WHZ WAZ	1178 children 0 to 59 months	Arua, Uganda	Stepwise multiple regression	Child's age, breast-feeding incidence, diarrhea/skin infection incidence, mother's education, father's education, dry season water source	1
(Vella et al., 1994)	Stunting — —	827 longitudinal cohort over 2-year interval	Arua, Uganda.	Logistic regression	Child's age, mother's education, income	1
(Biondi et al., 2011)	Stunting — —	299 children 6 to 59 months	Kabarole, Uganda	Logistic regression	Caregiver's health, water source contamination, household economic status, health unit distance, residence	1
(Ssewanyana & Kasirye, 2012)	HAZ — —	12,035 young children	Uganda	Linear regression	Mother's education, household welfare status	2b
(Muhoozi et al., 2016)	HAZ WHZ WAZ	512 children 6 to 8 months	Kabale and Kisoro, Uganda	Chi-square/Pearson's correlation tests and linear regression	Child's sex, birth order, diet diversity score, mother's age/education, household head's education, sanitation facilities, household size/poverty	1
(Shively, 2017) [‡]	HAZ WHZ —	4345 young children	Uganda	Hierarchical analysis	Agricultural season rainfall, health infrastructure, transportation infrastructure, heterogeneity across household/district/region levels	3
(Yang et al., 2018)	HAZ/Stunting — —	14,747 children under 5 years	Uganda	Univariable, bivariable analyses, and multi-variable logistic and linear regression	Child's age/sex, birth size, mother's age/education, household wealth, region	2a
(Nankina et al., 2019)	Stunting Wasting Underweight	3531 children under 5 years	Uganda	Chi-squared tests and multivariate logistic regression	Child's age/sex, birth weight, mother's age/education/occupation	1
(Manda et al., 2016)	Stunting — —	810 children 0 to 60 months	Zambia	Endogenous switching probit regression	Child's age/sex, household head's sex, female household member's count/education, sanitation access, improved maize variety adoption	2b

(Griffiths et al., 2004) [‡]	— — WAZ	Children 1 to 35 months (2050; 4083; 3237; 2042; 1803; 3485)	Multiple: Ghana, Malawi, Nigeria, Tanzania, Zambia, Zimbabwe	Multilevel analysis	Child's age, birth size, breast-feeding status, diarrhea incidence, mother's education, heterogeneity across family/community/region levels	2a
(Makoka & Masibo, 2015)	Stunting Wasting Underweight	Children 0 to 59 months (4563; 482; 3473)	Multiple: Malawi, Tanzania, Zimbabwe	Bivariate and multivariate logistic regression	Mother's education	1
(Mosites et al., 2015)	Stunting — —	Children under 5 years (8720; 4203; 1740)	Multiple: Ethiopia, Kenya, Uganda	Log-binomial regression	Livestock units	1
(Hoffman et al., 2017)	Stunting Wasting —	Children under 5 years (5478; 5150; 5088; 5478; 5150; 5088; 11,335)	Multiple: Kenya, Zambia	Multiple linear regression, logistic regression	Mother's education/literacy, electricity use, toilet type, car ownership, household wealth, region	2a
(Buisman et al., 2019)	Stunting — —	Children 0 to 23 months (4993; 1845; 4795; 2084; 1015; 2875; 2603)	Multiple: Ethiopia, Ghana, Kenya, Liberia, Namibia, Niger, Rwanda	Least squares regression	Child's age/sex, immunization status, iron supplement use, deworming incidence, diarrhea incidence, mother's height, maternity care, parental education, household wealth	2a

[‡] denotes study with hierarchical methodology

Most studies, 83% (n=153), involved analysis of a discrete malnutrition outcome variable (e.g., stunting, wasting, or underweight), while 15% of studies (n=28) involved analysis of a continuous malnutrition outcome (e.g., HAZ, WHZ, or HAZ), and only 1.6% of studies (n=3) undertook analyses of both. Across 10 countries from 2004 through 2019, an 11% sub-section of studies (n=21) incorporate hierarchical methods including random intercepts and slopes, and intraclass correlation analysis. Of the three major anthropometric indices and indicators, 168 studies (91%) examine stunting or HAZ, 96 studies (52%) examine wasting or WHZ, and 83 studies (45%) examine underweight or WAZ (Figure 3).

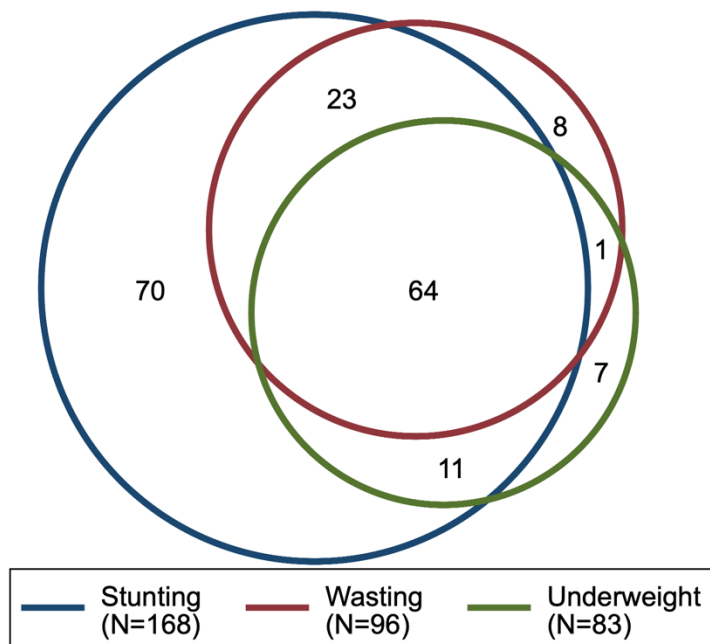


Figure 3: Proportional Venn diagram of studied nutritional outcomes. Indicator labels refer to studies of either discrete or continuous outcomes.

Stunting is disproportionately represented in the literature. Studies that assess stunting alone comprise 38% of papers (n=70) compared to 4.3% (n=8) for wasting alone and 3.8% (n=7) for underweight alone. Of the possible combinations of

malnutrition outcomes, the most prevalent is all three—stunting and wasting and underweight—comprising 35% of studies (n=64), followed by stunting and wasting alone with 13% of studies (n=23). Stunting and underweight alone account for 6% of studies (n=11) and the combination of wasting and underweight is only observed once in the literature.

During the first decade from 1990-1999, only 10 studies appear in the literature (Figure 4). From 2000-2009 there is a stable but substantial upswing in the number of publications, with an average of 3.3 publications per year. Starting in 2010, the literature experiences an exponential growth in the number of studies being published, with 2015 being a particularly significant inflection point for the increase in the number of studies. Over half of the total number of studies (n=107) were published during the last five years.

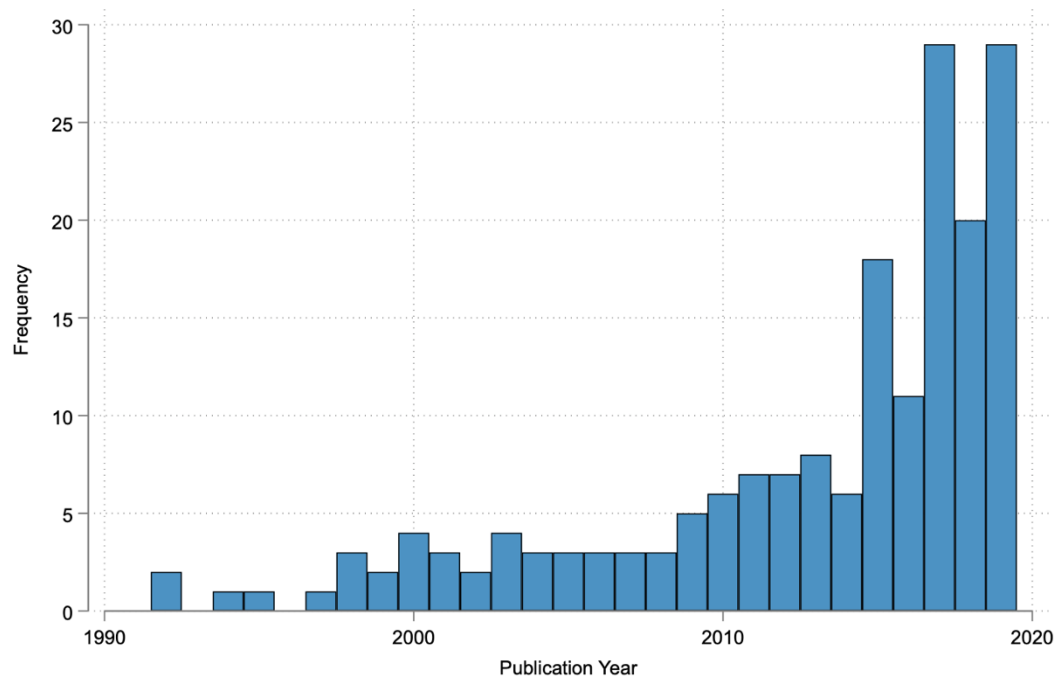


Figure 4: Temporal distribution of the literature

The top four most studied countries—Ethiopia (n=47), Nigeria (n=23), Ghana (n=15), and Tanzania (n=15)—account for half of the total number of studies (Figure 5). The remaining 100 studies are spread across 30 countries (Angola, Benin, Botswana, Burkina Faso, Burundi, Cameroon, Chad, Congo, D.R.C., Egypt, Gambia, Guinea, Guinea-Bissau, Kenya, Liberia, Libya, Madagascar, Malawi, Mali, Mozambique, Namibia, Niger, Rwanda, Senegal, Somalia, South Africa, Sudan, Uganda, Zambia, and Zimbabwe). Regionally, 5% of studies (n=10) are from Central Africa, 65% of studies (n=119) are from Eastern Africa, 3% of studies (n=5) are from Northern Africa, 8% of studies (n=14) are from Southern Africa, and 28% of studies (n=52) are from Western Africa. Five studies (i.e., Buisman et al., 2019; Griffiths et al., 2004; Hoffman et al., 2017; Makoka & Masibo, 2015; Mosites et al., 2015) contain multiple individual country analyses (n=21).

In 13 countries—Burundi, Niger, Libya, Mozambique, Chad, Sudan, Benin, Congo, Gambia, Guinea, Guinea-Bissau, Liberia, and Namibia—only one study can be found in the literature. Among these 13 single study countries, 7 are among the top 20 countries with the highest stunting prevalence rates (UNICEF et al., 2021), including Burundi (1st), Niger (3rd), Libya (4th), Mozambique (8th), Chad (13th), Sudan (15th), and Benin (20th). Among the top 20 countries with the highest stunting prevalence rates (UNICEF et al., 2021), Eritrea (2nd), Central African Republic (7th), Djibouti (14th), and Lesotho (18th) are unstudied in this literature. Wasting and underweight is so much less studied that reliable and consistent, country level, continent wide, up-to-date prevalence rates are not reported.

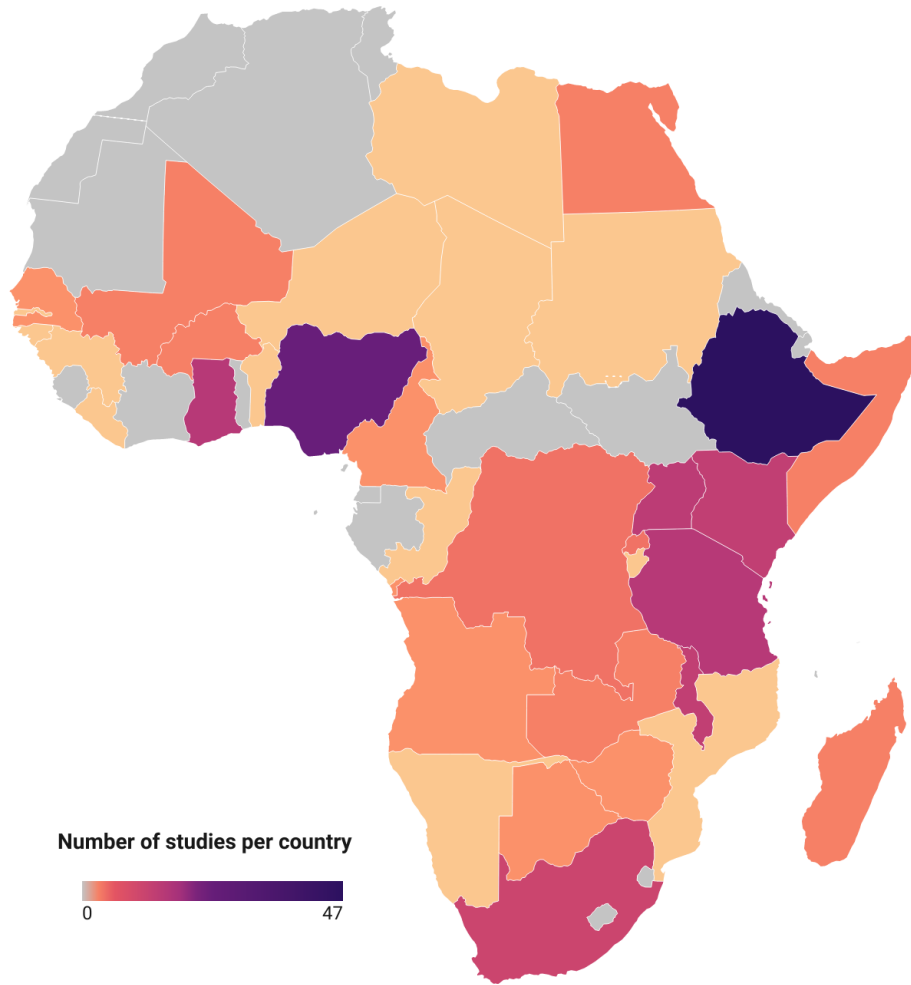


Figure 5: Map of empirical literature coverage by country

Together 28% of studies (n=52) utilize Demographic and Health Surveys (DHS) Program data. The DHS Program has collected health data of some kind in a total of 46 African countries. Countries with DHS data in the literature include: Burkina Faso (n=2), D.R.C. (n=3), Egypt (n=1), Ethiopia (n=10), Ghana (n=10), Guinea (n=1), Kenya (n=7), Liberia (n=1), Madagascar (n=1), Malawi (n=4), Mali (n=1), Namibia (n=1), Niger (n=1), Nigeria (n=9), Rwanda (n=3), Tanzania (n=4), Uganda (n=5), Zambia (n=2), and Zimbabwe (n=2). A total of 20 countries have available DHS data which have been unutilized by the literature, and 10 countries

with available DHS data are left unstudied altogether, including: Central African Republic, Comoros, Cote d'Ivoire, Eswatini, Gabon, Lesotho, Morocco, Sao Tome and Principe, Sierra Leone, and Togo.

The extant group of selected articles were published by a total of 36 publishers. Just four publishers (BMC, Cambridge University Press, Public Library of Sciences, and Wiley-Blackwell) account for a majority of the articles (n=100). Articles from blatant deceptive or predatory scholarly publishers were screened out. However, many included studies came from less than immaculate publishers and journals (see Beall, 2017; Strielkowski, 2017, 2018). A sub-group of the largest questionable publishers (BMC, Hindawi, MDPI, and Dove Medical Press) account for 36% of the literature (n=66). More attention should be made on the standards and quality of results in systematic reviews and meta-analyses.

3.6 Summary of Emergent Etiological Themes

Determinants are selected if present in more than three studies, which sets the lower bound on what quorum is necessary to suggest a common theme. Grouping is by proximal and distal factors, order of pervasiveness:

Table 2. Common determinants

<i>Reported study findings</i>	<i>Number of studies</i>
child's feeding	64
child's age	64
child's sex	60
child's birth size and weight	49
child's diarrhea incidence	31
child's vaccination, immunization, and deworming status	16
child's birth interval and order	15
child's fever incidence	12
child's general illness and infection incidence	9
child's cough incidence	5
child's twin status	5
mother's education and literacy level	76
mother's Body Mass Index	22
mother's age	19
mother's height	16
father's education level	15
parental occupation	13
parental and household head education level	7
household's wealth, income, and socio-economic status	56
household's water source and usage	25
household's latrine utilization and sanitation facilities	10
regional effects	13
urban and rural residence effects	13
seasonal effects	7

3.7 A Quality and Quantity Assessment

Causal identification of nutrition etiologies is difficult; it cannot be done with careless regressions (Buisman et al., 2019). A gap exists in the literature of studies that measure the effect sizes of possible non-illness related malnutrition interventions. I apply a simplified version of the 19-part “questionnaire” developed by McCloskey and Ziliak (1996; 2004b) for two literature reviews in the economics field. I similarly evaluate the state of null hypothesis significance testing, and other limitations within

the child malnutrition literature (Nickerson, 2000). The objective is to assess the quality of the study's approach and conclusions. This "Size Matters" approach determines if results are based in statistically significant terms alone or if the results have any epidemiologically significant justification. Similar questionnaires have been adopted in literature reviews of conservation biology (Fidler et al., 2006), criminology (Bushway et al., 2006), and psychology (Sun et al., 2010).

In the child malnutrition literature, I looked for studies that explicitly distinguished epidemiological significance from statistical significance. This includes articles that reported the effect size of a determinant and interpreted the effect size by placing it with a broader scientific conversation about what effects would be judged "large" or "small". Other criteria included studies that motivated their coefficient selection without p-hacking or methods lacking in scientific judgement and studies that avoid inappropriate or otherwise spurious statistical tests. A note was made if a study reported the power of their tests and the findings. Similarly, findings based on a small number of observations, such that no statically significant differences can be found, or on a large number of observations, such that statically significant differences are arbitrary, are also noted.

I find that papers in this literature are spread across three general categories: (1) those without any presentation of epidemiological significance or effect size; (2a) those that do present some quantitative effect sizes of some kind, however, only after first explicitly p-hacking their coefficient selection or (2b) those that do discuss some effect sizes, but without context to judge epidemiological effects and only after identifying meaningful determinants from post estimation tests of statistical

significance or (2c) those that do discuss epidemiological significance and effect sizes but only of statistically significance determinants; and (3) those in which statistical significance was not the primary driver behind the results and coefficient selection was not derived from p-hacking procedures; instead scientific judgement is applied and explicit quantitative results are discussed and presented within a larger context of epidemiological significance.

Across the entire body of literature, 59% of studies (n=109) fall into the first category, and 39% of studies (n=72) fall into the second category: with 18% (n=33) in category 2a, 18% (n=33) in category 2b, and 3% (n=) in category 2c. Only 1.6% of studies (n=3) fall into the third category. However, there are a number of well-reasoned and informative studies, just below the “Size Matters” cutoff (see Abuya et al., 2012; Begin et al., 1997; Fotso et al., 2012; Gewa & Yandell, 2012; Grace et al., 2012; Haile et al., 2016; Kabubo-Mariara et al., 2009; Ruel et al., 1999; Ukwuani & Suchindran, 2003) Articles within the third “Size Matters” category include Kinyoki et al. (2015), Kinyoki et al. (2017), and Shively (2017).

Kinyoki et al. (2015) and Kinyoki et al. (2017) studied risk factors of stunting, wasting, and underweight for 73,778 young children in Somalia. Employing household cross-sectional national nutrition survey data and Bayesian hierarchical spatio-temporal regression analysis, they find fever, diarrhea, sex and age of the child, household size, access to foods, enhanced vegetation index, and conflict are significant predictors of malnutrition. Important predictors associated with wasting and stunting exhibited substantial regional variation. Diarrhea was associated with increases of 0.35 and 0.29 in wasting and stunting odds. Girls had 0.27 lower odds of

being either stunted or wasted. A 1-unit increase in enhanced vegetation index was associated with a 0.38 and 0.49 reduction in wasting and stunting odds. Recent conflict increased odds by 0.37 and 0.21 of wasting and stunting. Longer term conflict had even larger effects, increasing odds by 0.76 and 0.88 for wasting and stunting.

Shively (2017) studied the determinants of weight-for-height and height-for-age z-scores for 4,345 children under 5 years in Uganda. Employing the Uganda DHS-V and DHS-VI with a hierarchical analysis, he finds agricultural season rainfall, health infrastructure, and transportation infrastructure are significant factors of malnutrition. In the hierarchical framework, he finds heterogeneity at the regional, district, and household level.

Since 98% of the literature does not firmly pass the “Size Matters” metric, it is a futile exercise to synthesize the findings from such a collection of studies. It is invalid to posit that any real knowledge can be gleaned from tallying the results of size-less studies. Mere statistically significant positive or negative relationships between child malnutrition and various factors does not pass muster. It would be incorrect and even unethical to suggest otherwise. To make policy recommendations based on a determinant being qualitatively either a risk or a mitigating factor is dubious. Pseudo-analytic synthesis based on arbitrary levels of significance from a size-less literature misses the epidemiological point: they are neither informative nor meaningful. As Kenneth Rothman put it writing for the journal *Epidemiology*:

Omit tests of statistical significance ... discourage this type of thinking. ... We also would like to see the interpretation of a study based not on statistical significance, or lack of it, for one or more study variables, but rather on careful quantitative consideration of the data ... consider the magnitude of an

estimated effect ... rather than simply offer uninspired interpretation that the estimated effect is “significant.” ... As it only has two values, “significant” or “not significant,” it cannot convey much useful information. ... Misleading signals occur when a trivial effect is found to be “significant,” ... or when a strong relation is found to be “nonsignificant.” (Rothman, 1998, p. 334)

Reliance on statistical significance alone may lead to ignoring large but imprecise factors and highlighting precise but small determinants. The result is also a literature devoid of any actual findings. The optimistic perspective, however, is one where there are bountiful, untapped opportunities to develop this literature much further. A great opportunity rests within studies with quantifiable results: a new regime of explicit measurement of *how much* potential determinants impact child malnutrition outcomes. There exists a wealth of untapped potential for future discoveries of researchers aptly employing the “rigorous methods of science” (Goodchild, 2009). But it is a sad state of affairs for the millions of children who will continue to suffer because we, as a scientific community, continue to be satisfied with fooling ourselves.

3.8 Study Limitations

All systematic reviews suffer from over confidence in results (Arksey & O'Malley, 2005). The potential for mischaracterizing the study universe and introducing errors of omission is significant. In an initial trial search, I identified a much broader and extensive universe of 13,893 potential manuscripts. (For details of the expanded search criteria, see section 7.1.2). However, the burden of selection and review was too great. The missed potential for a more complete and comprehensive study is certainly not insignificant. However, casting this much wider net in an attempt to

catch elusive articles also undoubtedly ensnared many more non-pertinent articles at diminishing marginal return.

The study population is derived from only a single database (i.e., Web of Science) plus key journal hand searching with Google Scholar. Some pertinent journals, such as *Nutrition and Health*, *Tanzania Journal of Health Research*, and *The Nigerian Postgraduate Medical Journal*, are not indexed by Web of Science. Nor does Web of Science index a comprehensive range of publication dates for all journals that it does index (e.g., *South African Journal of Clinical Nutrition*, *African Journal of Reproductive Health*, *East African Medical Journal*, *BMC Research Notes*, *International Quarterly of Community Health Education*, and *International Journal for Equity in Health*).

The final selection of studies consists of only English language documents, which may lead to selection bias. In assessing the selected literature, the list of determinants does not include null results, or determinant effects that found to have no impact. The spatial scope of the review excludes all non-African countries. Asia along with Latin America and the Caribbean have significant child malnutrition prevalence, too. The review does not include all possible nutrition outcome assessments (e.g., MUAC) nor does it consider studies with composite outcomes (double burden of malnutrition). Studies with non-tractable analytical methodologies and aggregate or pooled studies that include more than one country were also excluded (e.g., Cooper et al., 2019; Kandala et al., 2009; Smith & Haddad, 2000).

However, despite these limitations, this review is the largest and most comprehensive of its kind. Even with its focused spatial, temporal, and

methodological selection criteria, the number of included articles is over twice as many as the next largest literature review of child malnutrition studies.

3.9 Conclusions

Since the introduction of the 1990 UNICEF conceptual framework there has been an escalation of corroborating empirical studies. Previous systematic reviews have attempted to synthesize the literature and identify various determinants (e.g., Blessing J Akombi et al., 2017; Brown et al., 2020; Keino et al., 2014; Phalkey et al., 2015). However, no other study is as comprehensive (including over twice as many papers as the next largest systematic review, despite having a more focused spatial, temporal, and methodological selection criteria). Nor has any other review shown how much the literature abuses tests of statistical significance.

I find most studies follow a typical structure, however, there remains little consensus of determining factors across time, space, and scale. Very few studies consider conflict and environment etiologies despite being the primary factors attributed to malnutrition, hunger, and death in most catastrophic famine events. Despite an extensive body of research, I find there are numerous opportunities for development within this corpus.

The first opportunity exists in the heterogenous patchwork of malnutrition research across time, countries, and scales. Over 38% of countries on the African continent are not represented at all in this field, while only 16 countries can point to more than two studies across the 30-year timespan. Nationally representative studies make up only 38% of studies, and an even smaller 29%, account for the heterogeneity of social experiences, across just 8 countries. Despite over half of the extant literature

being published in just the last five years, over half of these studies are from just one of three countries (Ethiopia, Tanzania, and Ghana). The disparate coverage raises doubts about the generalizability and operational usefulness of many established paradigms and heuristic approaches.

The second growth opportunity underscored by this literature exists within the proliferation of stunting related malnutrition outcomes. Stunting related outcomes are studied in over 91% of the literature. Studies of stunting alone constitute 38% of the literature. Focusing solely on stunting is an error, one adopted out of convenience (Perumal et al., 2018). Some point to greater data availability and greater prevalence rates as a rational (Black et al., 2013; de Onis & Branca, 2016; Smith & Haddad, 2015; UNICEF, 2013). Studying stunting because of greater data availability and greater prevalence rates, however, is the quintessential *drunkard's search principle*—an observational bias that occurs when one only searches for something where it is easiest to look. I hope the child malnutrition research community sobers up to such drunken temptations.

The third and most bountiful growth opportunity in the literature resides with developing quantifiable results: the explicit measurement of how much each determinant impacts malnutrition, especially for non-illness related determinants. I find that, overall, the literature lacks the capability to answer the simple question: How much does any particular determinant effect malnutrition prevalence? Of the 184 papers using a test of statistical significance, fully 98% mistook a merely statistically significant finding for an epidemiologically significant finding.

What matters for scientific advancement and meaningful practical execution is the impactfulness of a determinant. Impactfulness explains how much a determinant is practically useful even if it is imprecisely measured (McCloskey, 1995). Confusion over statistical and substantive significance often leads to misinterpretations, devoid of actual *scientific* findings (Goodman, 2008; Greenland et al., 2016; Wasserstein & Lazar, 2016; Ziliak & McCloskey, 2008). Such confusion is rampant in the empirical disaggregate African child malnutrition literature. Indeed, other systematic reviews have found that much of the evidence for the impact of climate on childhood malnutrition is based on a few heterogeneous studies with flawed methodologies (Phalkey et al., 2015). Despite widely anticipated links between climate change and child malnutrition, evidence for the nature of the relationship is just beginning to emerge across expansive spatial and temporal scales (Niles et al., 2020). More studies are needed, with more geographic coverage, and more attention to scale, that include multiple dimensions of nutrition outcomes, and are couched in sound inferential theory to quantify the spatial, social, political, climatic, and economic determinants of malnutrition.

4 On the Quality Control Maxim of Standard Deviations

4.1 Overview

Anthropometry is the study of the measurements and proportions of the human body. In the field, many practitioners have adopted a questionable quality control maxim. The maxim is, essentially, to dismiss any survey of anthropometric measurements whose standard deviation exceeds that of a benchmark survey, sample, or distribution (e.g., by 1.3x). To date there is no published study which properly substantiates the maxim. Despite the lack of sound statistical justification and lack of scientific evidence, the standard deviation as quality control indicator persists. Practitioners who endorse the maxim transpose the conditional and muddle samples with populations and references with standards. The practice is endemic and may have real consequences in terms of financial resources and global morbidity and mortality. This essay details the genesis and propagation of the maxim in the literature, exposes its theoretical and logical weaknesses, illustrates its demerits, and offers an alternative attitude toward the problem of quality control.

4.2 Exordium: $SD \neq QC$

Anthropometry is the study of the measurements and proportions of the human body. It is widely accepted that for practical purposes anthropometry is the most useful tool for assessing the malnutrition status of children (WHO, 1986). Malnutrition is responsible for 45 percent of all deaths among children worldwide (Black et al., 2013). In 2017, acute malnutrition (wasting) menaced over 50 million young children

while over 150 million young children suffered from chronic malnutrition (stunting) (UNICEF et al., 2018). Even a small change in child malnutrition rates can have major consequences in terms of lives saved or lost. The financial and human costs associated with the practice of anthropometry can be enormous. In 2014 alone, global donors disbursed nearly \$937 million in nutrition-specific programming (KFF, 2016). According to Meera Shekar et al. (2017), to achieve the World Health Assembly global nutrition targets, the world needs to invest \$70 billion over 10 years in high-impact nutrition-specific interventions.

The two most widely studied expressions of anthropometric indices are weight-for-height (WHZ) and height-for-age (HAZ) z-scores (de Onis & Blössner, 1997; de Onis & Habicht, 1996). These z-scores express anthropometric measurements in terms of standard deviations below or above a reference population value. A z-score is the difference between a particular child's measurements and the mean value of comparable children from a reference population, divided by the standard deviation of that reference population (WHO, 1995). Z-scores require a well specified reference population with a normal distribution, a condition which would imply that z-score cutoff values for stunting, wasting, or underweight are stable across different reference populations.

However, many practitioners operate under the assumption that the standard deviation (SD) of a survey's anthropometric indices is a necessary and sufficient measurement for quality control (QC). Exactly how many is up for debate and a potential direction for future research. Suffice it to say the number is large. If one is unfamiliar with this particular body of literature or the day-to-day pragmatics of

organizations working in this field, then the SD as QC problem might not seem endemic. But much like dust in the air, to borrow a metaphor, SD as QC seems invisible — even if you’re choking on it — until you let the sun in. Then you see it’s everywhere. A collection of quotes from this search is provided in section 7.2.1 to help illuminate the extent, certainly representing only a small sub-sample of all the potential articles and reports. Not to mention the many unreported, unknown, and unknowable studies that never saw the light of day because of internal or external suppression for having a supposedly over-large standard deviation.

The practice is particularly persistent for anthropometric surveys within the field of child malnutrition, with particularly grievous consequences. In one typical article, the quality control maxim for z-scores states, “summary statistics can be compared with the reference, which has an expected mean Z-score of 0 and a SD of 1.0 for all normalized growth indices” (Mei & Grummer-Strawn, 2007, p. 441). Others suggest that if a survey presents with “an excessive standard deviation ... the survey results should be rejected” (Grellety & Golden, 2016). The maxim is certainly simple, but does its simplicity compensate for its disadvantages?

Suppose you wish to conduct an anthropometric survey across the Karamoja region of northeast Uganda, to assess the health of the region’s children. Your well-designed survey includes measurements of height, weight, and age from a sample of children. You combine the measurements to make anthropometric indices of health such as weight-for-height and height-for-age. After performing some rudimentary summary analysis, you discover the sample standard deviations of the survey indices are (for example) 1.3 times greater than those of the 2006 World Health Organization

(WHO) reference standards, which is not surprising given that the two groups of children come from two distinctly different populations. However, the quality control maxim used by many anthropometric researchers would dismiss your Karamoja survey as low quality, simply because the standard deviations are 1.3 times greater than the 2006 WHO reference standards.

Anthropometric research generally works with z-scores, however, and the practice that I am objecting to is expressed in terms of z-scores, not sample standard deviations. Couched in terms of z-scores, the nature of the putative quality control requirement is a bit harder to understand. But it is really as simple as the Karamoja example: the ratio of standard deviations (of the sample and a reference) when in excess of a fixed threshold (e.g., 1.3) fail the quality control test. It can be shown that an anthropometric survey has a z-score standard deviation of 1.3 (or any other arbitrary cutoff value) *if and only if* the sample standard deviation of the anthropometric index is 1.3 times that of the standard deviation of the reference population. From a mathematical standpoint, a claim about the standard deviation of a z-score is equivalent to a claim about the ratio of an index's sample standard deviation to that of a reference population. For a proof, see section 7.2.2.

The notion that I wish to challenge is the following: Any anthropometric survey and subsequent z-score index (e.g., height-for-age or weight-for-height) not normally distributed with a standard deviation of approximately 1.0 (e.g., 1.3), indicates a serious problem, and should be considered unusable. (For more on the size and specifics of the maxim, see section 7.2.3). And I suggest there is neither statistical justification nor scientific evidence that supports the SD as QC maxim.

There are, of course, inaccurate surveys that probably deserve to be dismissed. Garbage in, garbage out. I too am wary, but other tests and conditions must be applied. For example, the United States Agency for International Development (USAID) identify 26 potential indicators that could measure anthropometry data quality during fieldwork (Allen et al., 2019). The World Bank and WHO recommends considering several indicators such as population characteristics, sample size, survey design, measurement methods, and missing data (Kostermans, 1994; WHO, 1995). WHO and UNICEF (2019) suggest performing a seven-point data quality assessment, which interprets and reports: completeness; sex ratio; age heaping; height and weight digit preference; and z-score implausibility, standard deviations, skewness and kurtosis. And Nandita Perumal et al. (2020) have implemented this suggestion to its fullest potential.

Emmanuel Grellety and Michael H. Golden (2016) highlight random measurement, digit preference and rounding error as potential sources of error. David A. Siegel and Jacob S. Swanson (2004) warn against heaping and digit preference. Researchers should also look out for confounding effects, specification error, non-linearity, bias of the auspices, measurement error, experimental error, and sample selection bias. Others point out that there is not even a consensus in the literature as to what constitutes a usable dataset (Crowe et al., 2014; USAID, 2016; Waterlow et al., 1977). Shireen Assaf, Monica T. Kothari, and Thomas W. Pullum (2015) say the need for well-defined quality assessment criteria remains unmet, and recommend more training and better equipment in the meantime.

In their methodological guidelines for assessing nutrition in crisis situations, the SMART (Standardized Monitoring and Assessment of Relief and Transitions) inter-agency initiative recognized that survey samples do *not* follow reference standards, and that even “the standard population is not normally distributed” (2006, p. 24). Later, however, the guidelines rely on the SD as QC maxim, claiming bias “can be estimated from examination of the standard deviation of the WFH, which should always be 0.8–1.2 z-scores” (SMART, 2006, p. 38).

Inspection of surveys for small SD remains in many QC recommendations (e.g., Allen et al., 2019; SMART, 2006; WHO & UNICEF, 2019) as a necessary if not sufficient condition for acceptance, while for others it is even a sufficient condition (e.g., Bilukha et al., 2020; Grellety & Golden, 2016, 2018; Mei & Grummer-Strawn, 2007). I propose that SD is neither a necessary nor sufficient indicator of QC. Low-quality surveys can have small SD and high-quality surveys can have large SD. Errors of commission and omission waste precious resources that are already spread thin. The disregarding of surveys with high standard deviation could result in funds and research being syphoned away from the people most in need. It is my aim to illustrate the archival, statistical, logical, theoretical, and practical evidence that standard deviation should serve as neither a necessary or a sufficient arbiter of quality control.

4.3 Narratio: Unsound Beginnings

It was sculptors and painters who first measured the relative proportions of the human form (Tanner, 1981). Scientific study of the measurements of the human body emerged notably with the work of Adolphe Quételet in 1832. Much like

contemporary practitioners, Quételet performed a cross-sectional study of the height and weight of newborns and children, and observed a likeness between the distribution of weight and height to a normal (Gaussian) distribution (Quételet, 1832, 1835). This Quételet Index, later redubbed Body Mass Index, is still relevant today. Unlike Quételet, however, contemporary practitioners have transposed his observation, and adopted the quality control practice of judging a survey based on its likeness to a standard normal distribution.

The source of the misconception originates in a presentation at the 15th International Congress of Nutrition in 1993 by Ray Yip. Despite its later impact on the literature, the SD-as-QC proposal does not even appear in the summary of the workshop, including Yip's abstract (Yip, 1993). However, two years later the WHO issued a technical report entitled *Physical status: The use of and interpretation of anthropometry* that many have cited as the originator and authority for the SD as QC maxim.

In less than one page of a 463-page report, some of the most recurrent maxims are found. WHO (1995) outlines several steps involved in assessing the quality of anthropometric data, including the observed standard deviation of the z-score distribution. With accurate measurements, the report claims the “distribution should be relatively constant and close to the expected value of 1.0 for the reference distribution” (WHO, 1995, p. 218). Citing the 1993 conference abstract, the report presents a table of “the standard deviations of the height-for-age, weight-for-age, and weight-for-height z-score distributions” all ranging “within approximately 0.2 units of the expected value” (WHO, 1995, p. 218). The table of values include: HAZ (1.10 to

1.30), WAZ (1.00 to 1.20), and WHZ (0.85 to 1.10). The expected value of 1.0, the range of plus or minus 0.2 units, and the specific table values have all been widely cited as the criteria which constitute a good quality survey (e.g., Bilukha et al., 2020; Blanton & Bilukha, 2013; de Onis & Blössner, 1997; Grellety & Golden, 2018; Mei & Grummer-Strawn, 2007; SMART, 2006; WHO & UNICEF, 2019).

WHO (1995) presents the table of SD ranges only as an *example* that was observed during multiple large-scale CDC surveys presented once at a conference. The range of plus or minus approximately 0.2 units is merely a generalization they ascribe to the example surveys. In fact, WHO (1995) goes on to say that in some surveys the observed standard deviations ranged from 1.4 to 1.8, even after excluding extreme outliers. The specific SD values were *not* given in WHO (1995) as QC ranges as many have claimed (e.g., Castro Bedriñana & Chirinos Peinado, 2014; Grellety & Golden, 2018; Gupta et al., 2020; Jacob et al., 2016; Kwená et al., 2003; Mei & Grummer-Strawn, 2007; Wijaya-Erhardt, 2019).

The report does suggest a $SD > 1$ *could* be an indicator of inaccuracy, but the notion was couched in a larger discussion of indicators, including validity of the reference population, the notorious quality of age estimates, errors of rounding and digit bias, number of missing and improbable values, and overall data compilation and documentation. Standard deviation is but one potential indicator, of many, to flag surveys for further inspection, *not* a sufficient measure of quality (WHO, 1995). And the report recommends: “Verification of accuracy is best done by remeasurement of a sub-sample of the original sample by individuals who are fully qualified in

anthropometric procedures” (WHO, 1995, p. 216). In other words, standard normal SD is certainly not a sufficient QC condition.

Soon after, Mercedes de Onis and Monika Blössner (1997) echoed the SD as QC maxim as a definitive fact of nutrition surveys in their report *WHO Global Database on Child Growth and Malnutrition*, which many others have cited as the progenitorial charter of the idea. In particular, de Onis and Blössner claim:

If the surveyed standard deviation of the Z-score ranges between 1.1 and 1.2, the distribution of the sample has a wider spread than the reference. Any standard deviation of the Z-scores above 1.3 suggests inaccurate data due to measurement error or incorrect age reporting. (de Onis & Blössner, 1997, p. 51)

The first sentence is referring to the *survey* data compared to the *reference* data. It is only making general statements about how variance and spread can be described for any two distributions of data. The second sentence, however, jumps to the conclusion that a z-score standard deviation above 1.3 “suggests inaccurate data.”

Without question, z-score summary statistics can illustrate a broader community-wide picture of malnutrition; that is their function. As de Onis and Blössner state earlier “if a condition is severe, an intervention is required for the entire community, not just those who are classified as “malnourished” by the cut-off criteria (1997, p. 50). That is to say, when analyzing z-scores, *if* many observed z-scores are well below the reference, *then* one might conclude that the appropriate intervention mechanism should be aimed at the population, and not the individual level. This is a sensible, if tautological, suggestion. But the inverse is not necessarily true. Namely, if you do not observe a standard normal distribution of z-scores shifted

in mean only, then you conclude none of the population has been affected and the sample is simply of low quality.

It seems obvious that a population by definition will *not* move together as a whole. We know that low income families are more vulnerable to price volatility and uncertainty because they have fewer options, entitlements, and capabilities (Sen, 1984). Calorie elasticity is not zero (Subramanian & Deaton, 1996). These families have relatively little income and a large percentage is spent on food, making them more vulnerable, thus skewing the distribution asymmetrically.

Larger z-score SD implies larger spread implies inaccurate data: simple but unsatisfying. I have not found substantiating evidence or theoretical justification for the maxim—in de Onis and Blössner (1997) in particular or the literature in general. But what I have found is a history of citations built upon a shaky foundation.

In my estimation there are really only two studies which one could argue have attempted to show evidence or justification for SD as QC, if only tangentially. The first comes from a conference paper given at the *Proceedings of the Standardized Monitoring and Assessment of Relief and Transitions (SMART) Workshop, July 23-26, 2002*. At the workshop Michael H. Golden and Yvonne Grellety presented a working paper in which they claim to disprove the assertion: “social heterogeneity would lead to changes in the shape of the distribution curve of acute malnutrition when a population is exposed to famine” (2002, p. 3). And through their analysis they conclude that “there was no change in the spread of wasting within the population as it became more malnourished” (2002, p. 3). Grellety and Golden (2018) stipulate that

these findings confirm that SD should be between 0.8 and 1.2 z-score units in all well-conducted surveys.

The findings of the Golden and Grellety (2002) working paper rest largely on Kolmogorov-Smirnov tests. In this case, the null hypothesis claim is that heterogeneity of wasting (i.e., z-score distribution curve) is heteroscedastic and the goal of the test is to falsify that claim. Their objective is to prove distributional spread (i.e., SD) is independent, stable, and standard normal (i.e., close to 1.0) as populations are exposed to starvation and famine (i.e., changes in average z-scores). And as an extension of their Kolmogorov-Smirnov test, they suggest SD is a measure of QC, stating:

If a survey is observed to differ significantly from normality or have a large standard deviation, then we suggest that either two distinctly different populations may have been included in the sample or there is methodological error. All surveys should be checked for normality and any difference investigated. (Golden & Grellety, 2002, p. 10)

But the specific Kolmogorov-Smirnov tests that Golden and Grellety (2002) devise assumes the data are normally distributed from the start. In this case the null hypothesis is not heterogeneity, but that z-score distribution curves are in fact normal. Furthermore, Thomas Bayes (1763) shows us that it is incorrect to assume $\Pr(\text{Data}|\text{H}_0) = \Pr(\text{H}_0|\text{Data})$. And testing for normality is not equivalent to testing a unit SD. We are also not provided the power of the tests (i.e., the probability of correctly rejecting the null hypothesis), making it difficult for one to judge a null hypothesis false when it is false.

Finally, in their figures, they purport that mean and standard deviation are uncorrelated. But if two random variables are statistically uncorrelated, that does not

imply they are independent. But it is independence that they seek. In addition, they show that kurtosis varies from -0.75 to 1.75 decreasing as wasting escalates, and skewness varies from -0.5 to 0.75 increasing as wasting escalates, contradicting the claim that malnutrition prevalence remains fixed and normally distributed.

In my estimation, even if Golden and Grellety (2002) had shown what they intended, it is still a great leap to conclude that therefore standard deviations are a necessary and sufficient quality control measure. The link is missing. Many alternative hypotheses still exist. As Deirdre N. McCloskey and Stephen T. Ziliak point out, “Failing to reject does not of course imply that the null is therefore true. And rejecting the null does not imply that the alternative hypothesis is true: there may be other alternatives which would cause rejection of the null” (1996, p. 102). And elsewhere, Golden concedes that “[m]ost experimental studies do not include the acutely ill children for ethical reasons; the children are studied after they have recovered from acute infections and other major complications” (2009, p. S280). The esteemed pediatrician James Tanner knew in 1952 what remains true today: unhealthy populations could be non-Gaussian and skewed; as such, standard deviations may be biased and not locate the right points (Tanner, 1952).

The second study comes from an article by Zuguo Mei and Laurence M. Grummer-Strawn (2007). Mei and Grummer-Strawn claim to “assess whether the SD of height- and weight-based Z-score indicators derived from the 2006 WHO growth standards can still be used as data quality indicators” (2007, p. 441). They find, “The SD for all four indicators were independent of their respective mean Z-scores across countries” (Mei and Grummer-Strawn 2007, 441). And they conclude that, “the SD of

Z-scores could still be used as a data quality indicator for evaluation of anthropometric data” (Mei & Grummer-Strawn, 2007, p. 445).

Again, WHO (1995, p. 218) present a table of z-scores with different ranges of distribution values (i.e., HAZ (1.10 to 1.30), WAZ (1.00 to 1.20), and WHZ (0.85 to 1.10)). However, as I hope I have illustrated, the table is presented only as an *example* of observed ranges. And the standard deviation z-score ranges were never meant for data quality assessment, nor has SD ever been shown to be a sufficient QC indicator.

But the point is lost in Mei and Grummer-Strawn (2007), who submit that WHO (1995) recommended “standard deviation ranges for data quality assessment” and claim to assess “whether these Z-score ranges still apply.” I suggest they never did. Mei and Grummer-Strawn even concede that “the observed ranges of SD for all four indicators from our analysis were consistently wider than those recommended by WHO” (2007, p. 441). Yet these specific values were never given in WHO (1995) as the acceptable range for good quality surveys.

Citing WHO (1995), Mei and Grummer-Strawn assert that:

On the basis of the 1978 WHO/National Center for Health Statistics (NCHS) growth reference, WHO has previously indicated that the SD of Z-scores of these indicators is reasonably constant across populations, irrespective of nutritional status, and thus can be used to assess the quality of anthropometric data. (Mei & Grummer-Strawn, 2007, p. 441)

I think it is telling that they point to the 1995 technical report instead of pointing to the actual developers of the WHO/National Center for Health Statistics (NCHS) growth reference (e.g., Waterlow et al., 1977).⁵

In fact, the arbiters of the WHO/National Center for Health Statistics (NCHS) growth reference, John C. Waterlow et al., warn against universal principles, saying: “Decisions of this kind have to be taken locally, and it is not possible to make international recommendations about them” (1977, p. 491). Indeed, we need to make judgments backed up by logic, theory, and evidence, and not blindly follow a binary decision rule lacking any contextual nuance. Waterlow et al. affirm that sub-populations are heterogenous, imploring us to make judgments on a case-by-case basis:

Clearly, if there were differences dependent on different gene distributions, then the target for one population would not be the same as the target for another. ... Because the reference population cannot be used as a universal target, the question of what is a realistic goal in any particular situation does become important. (Waterlow et al., 1977, p. 490)

The purpose of Waterlow et al. was to “present recommendations for the analysis and presentation of height and weight data” (1977, p. 489), *not* to present ways to exclude such data. All constraints they do propose are wholly directed at

⁵ In 1971, as part of a long tradition for child growth references, the Maternal and Child Health Program, the United States Public Health Service, and the American Academy of Pediatrics concurred that more rigorous standards were needed for clinical characteristics of early childhood malnutrition. This decision was the impetus for the Health and Nutrition Examination Survey carried out by the Centers for Disease Control and Prevention’s National Center for Health Statistics Task Force. First released in 1977, the National Center for Health Statistics Growth Curves were a combination of data from the National Center for Health Statistics’ Health Examination Surveys, the Health and Nutrition Examination Survey, and the Fels Research Institute. Wanting in on the action, a WHO working group on nutritional surveillance made recommendations on the criteria for the anthropometric reference population and presented recommendations for the analysis of data from surveys involving nutrition and anthropometry, thus the “WHO/National Center for Health Statistics” growth reference.

constructing a *reference* population. Whereas a *standard* represents a desirable target or norm, the sole aim of a *reference* is to be a common basis in order to group, analyze, and compare different populations (WHO, 1995). Unfortunately, the distinction between references and standards was, and continues to be, indifferently heeded and oft left in unclarity.

The 1978 WHO/National Center for Health Statistics (NCHS) growth *reference* is distinct in its purpose and function from the 2006 WHO Multicentre Growth Reference Study (MGRS) growth *standards*. And neither can inform, through comparing standard deviations, whether or not any particular *sample* is of poor quality. But Mei and Grummer-Strawn assert that, “our analysis confirms the WHO assertion that the SD remains in a relatively small range for each indicator” (2007, p. 445). To do so, however, is to conflate *standards*, *references*, and *samples*.

In 1993, the Expert Committee on Physical Status, convened by WHO, concluded that previous *reference* growth charts had long been misconstrued as a *standard* for growth (de Onis & Habicht, 1996). As a result, the WHO Multicentre Growth Reference Study was implemented between 1997 and 2003. The designers of the new Growth Reference were intentionally *prescriptive* rather than *descriptive* (Garza & de Onis, 2004). They designed a growth chart for how children *should* grow rather than how children *actually* grow. In other words, it was purposely designed to produce an idealized *standard* rather than a baseline *reference*.

Even the initial sample data for the Multicentre Growth Reference Study did not have small and well-behaved standard deviations. To produce the growth standards, the sample was manipulated to fit specific distributional requirements

(WHO, 2006b). And even though the study sought out the healthiest, most ideal population to measure, 93 percent to 69 percent of the healthy populous were ineligible and did not conform to this ideal (for more on the The Multicentre Growth Reference Study, see section 7.2.4). In other words, even in the healthiest and most ideal sub-populations, most children do not fit the growth standards, nor are they normally distributed with standard deviations close to one. The Multicentre Growth Reference Study is a growth standard intended for measuring benchmark distances from an idealized healthy child. It is not the only permissible distribution for a sample dataset nor is it relevant for measuring data quality.

4.4 Probatio: Spurious Theory and Flawed Logic

SD as QC may be believed by some to be loosely related to the seminal concepts of the eminent epidemiologist Geoffrey Rose, whose ideas transformed the strategy of preventive medicine. Central to Rose's strategy was his assumption that the width of the distribution of a variety of biological measures remains similar across different populations even as the mean of the distribution shifts: a mean-centric view of population (Rose, 1992). He observed that most risk-factor distributions across populations appear to have uniform displacements, with risk changing the same amount at different parts of the risk-factor distributions. Rose's assumption implies that the mean of a distribution can be used as a proxy for a population's intrinsic traits.

Yet it remains an untenable leap to go from Rose's "distributions of biological measures tend to have consistent spread, independent from the central tendency" to the misconception that "any distribution of a biological measure that does not have a

‘small’ and ‘precise’ spread is invalid, inaccurate, and not inciteful.” Furthermore, Rose’s conceptualization is anchored on the cohesiveness of populations, an assumption that may be violated by differential changes in the BMI distribution occurring globally within populations (Razak et al., 2016).

Contrary to theoretical and observational expectations, some have claimed whole population distributions shift equally in the face of malnutrition stressors and that any data set which does not behave that way (i.e., any data set with z-score standard errors not equal to one) must be a low-quality survey (e.g., Bilukha et al., 2020; Blanton & Bilukha, 2013; de Onis & Blössner, 1997; Golden & Grellety, 2002; Grellety & Golden, 2016, 2018; Mei & Grummer-Strawn, 2007).

But the assertion remains unsubstantiated. If true, it would follow that whenever there was a famine (malnutrition stressor) anywhere in the world, you sitting at the breakfast table, drinking your coffee, oblivious to the famine, would also become slightly malnourished, too, to maintain a normally distributed population with a standard deviation of one. We all must move together to preserve the spread of the distribution, you see. “That is preposterous,” the SD as QC crowd say, “Mean shifts in z-scores do not occur for the entire planet, it is only applicable to some smaller sub-population.” Ah, then, by “shifts in the population,” they don’t really mean the Population. Okay, but they still have to contend with the problems of *sorites* and the fallacy of the transposed conditional (for more on the fallacy, see section 7.2.5).

If the effect is only valid for some sub-population then the boundaries of that sub-population must be defined. And, in defining that boundary, the sub-population is by definition not representative of the whole population. Indeed, the casual parlance

of “populations” should be avoided. The meaningfulness of descriptive statistics depends on how meaningfully a population is defined in relation to the inherent intrinsic and extrinsic dynamic generative relationships by which they are constituted (Krieger, 2012).

Prevalence and distributions of z-scores are therefore highly reliant on boundary definitions and cannot be extrapolated out of sample. Remember, too, that the “reference population” used for judging a child’s health is really a *standard* and by design a small sub-population of only the healthiest of healthy children. And, even still, those “standard” children were not distributed standard normal with a standard deviation of one (WHO, 2006b). There is no reason to believe that a healthy sub-population should behave the same way a malnourished sub-population does. In fact, we would expect differences, else our work to solve the problem of malnutrition would be trivial.

Standard deviation is merely the measure of dispersion for a set of values, unlike digit preference (heaping at 0 and 5), incompleteness (missing values), rounding errors (chop vs. nearest), data formatting (short, long, float double), transposition and transcription errors (obvious typos), or procedural errors (e.g., a child measured lying down when they should have been standing), which are all direct quality control metrics of a specific error. For example, the standard deviation of WHZ only gauges the ratio of the weight-for-height sample standard deviation to that of the weight-for-height standard deviation of a reference population. The reference population (even if it is a *standard*) cannot signify anything qualitative

about the sample data, nor should it. A reference population is merely a datum or a fixed point. It is a quantitative scale not a qualitative apparatus.

Measurement errors *might* generate inflated SD. Then again, they might not. Inflated SD does not necessarily imply measurement error (Biehl et al., 2013; Ulijaszek & Kerr, 1999). The advertised “test” for the quality of a survey poses the prior “if the population is distributed normal, then the observed data will be distributed normal,” and supposes wrongly “if data is observed, then the population it is drawn from is distributed normal.” If H , then O , does not affirm if O , then H . It is the same as thinking if a person is hanged, then he will probably die; therefore, if observing a corpse, then one should conclude he was probably hanged (Ziliak & McCloskey, 2008).

Random errors lower precision by inflating confidence intervals. Random error is but one of many dozens of errors and seldom the biggest (Ziliak & McCloskey, 2008). It is systematic errors that we should be worried about. They cause bias. Especially when the costs of failure (i.e., child mortality) are high, the choice between low bias or low precision is not really a choice at all. If I can’t be precisely right, I would rather be generally right than precisely wrong. More importantly, Ziliak and McCloskey note “sampling precision says nothing about the oomph of a variable or model” (2008, p. 25).

Systematic errors may even attenuate SD. A small spread in SD is not a necessary condition for a lack of systematic error, making SD a poor metric from which to judge quality. Suppose, for example, I performed an especially erroneous survey of child anthropometry in which instead of actually measuring different

weights and heights, I just marked down the exact same value for every survey participant. Is my systematic measurement error captured by an inflated standard deviation? No. Obviously, this is an extreme and absurd example. But there exists a non-zero proportion of the total sample space in which systematic errors diminish rather than inflate standard deviation. Try to imagine the countless number of possible surveys with less extreme systematic error structures, all of which exhibit ‘a standard deviation of approximately one.’ If it is systematic errors that we are concerned with, SD signifies very little.

The obverse problem with SD as QC remains, too. Since *Anscombe's quartet* and the more recent *Datasaurus Dozen*, students of statistics have long known that different datasets with wildly varying graphical distributions can all have the exact same descriptive statistics, including standard deviation (Anscombe, 1973; Matejka & Fitzmaurice, 2017). Logic dictates SD is neither a necessary nor sufficient indicator of QC.

4.5 Refutatio: Informed Dissent from the Maxim

The debate surrounding standard deviation as quality control metric is ongoing and unresolved. After two national nutrition surveys in Nigeria exhibited divergent estimates, both USAID and UNICEF staff in-country felt that substantial quality problems must exist in either one or both surveys (USAID, 2016). In July 2015, the USAID Nutrition Division convened a technical meeting aimed at resolving the issues of accuracy and comparability of anthropometric data. Participants included representatives from USAID, CDC, UNICEF, WHO, PAHO, and external nutrition experts. The meeting report highlights that the importance of standard deviations for

measuring data quality was a major point of contention. The report concludes that “there was no agreement on what is a reasonable standard deviation of z-scores to expect in heterogeneous populations” (USAID, 2016, p. 17).

The meeting report features arguments for the SD as QC maxim given by an unspecified presenter from the CDC. In reference to the Demographic and Health Surveys, the CDC presenter asserted that high quality anthropometric data will *always* be normally distributed with a standard deviation of approximately one regardless of population heterogeneity, and that a standard deviation greater than one *must* mean the data are of poor quality (USAID, 2016). One example they pointed to was the National Health and Nutrition Examination Survey in the United States with a (recent) stable trend of small standard deviations. Furthermore, they claimed the shape of the distribution does not change as a population becomes more malnourished, concluding there is no relationship between the mean z-score and standard deviation. In their estimation, this lack of relationship is sufficient to conclude standard deviation is a quality control metric.

The report suggests, however, that not all participants agreed with the SD as QC maxim. Some participants felt that standard deviations greater than one could reflect heterogeneity in the population. For the Demographic and Health Surveys in particular, they expressed concern regarding the emphasis on standard deviations of height-for-age, weight-for-age, and weight-for-height z-scores close to one as an indication of quality. They noted:

In Kano state, Nigeria, for example, a majority of the within-cluster standard deviations were below 1, however, the average standard deviation in Kano state was more than 1. If the states are different, it is

impossible for the standard deviation to be 1 in every state, and 1 for the country as a whole. (USAID, 2016, p. 17)

Other researchers acknowledged that the Demographic and Health Surveys in particular did show the most variability in parameters such as standard deviation. But they also noted that the Demographic and Health Surveys Program has the largest number of surveys and covers the largest span of time; standard deviations may have changed with time as nutritional status of the populations changed or improved. One meeting facilitator affirmed that it is *not* true that the shape of the distribution does not change as nutritional status of the population changes. While others pointed out that in terms of the factors that influence anthropometric indicators (e.g., water, sanitation, and food security), the United States may be more homogeneous than other countries (e.g., India) (USAID, 2016, p. 16).

Given that standard deviations capture inherent population heterogeneity, there is no reason to assume that the standard deviation will be the same across all surveys. It is true that poor data quality could inflate the standard deviation of anthropometric measures, but given that anthropometric z-scores are biologic parameters, one would anticipate some population heterogeneity both within and between countries, even in situations of high-quality data collection.

The Joint FAO/WHO Expert Committee on Nutrition (1971) noted that statistical evaluation cannot by itself distinguish between what is normal and abnormal in the biological sense. Even seminal author and pediatric expert Dr. Derrick Jelliffe (1966) emphasized the problems and difficulties of non-sampling errors, which cannot be detected with tests of sampling errors. And Jonathan Gorstein

et al. (1994) notes that when the nature of a nutrition problem is unclear, it should be interpreted within the situational context.

Standard deviation is not indicative of quality control for some studies. There are researchers and journals confident enough in the quality of their findings even with standard deviations *not* approximately one. Yirgu Fekadu et al. (2015) found z-score standard deviations of 1.3 (weight-for-height), 1.33 (height-for-age), and 1.06 (weight-for-age) in Ethiopian children. Michel Garenne et al. (2009) found weight-for-height z-score standard deviations of 1.28 and 1.398 for Niakhar, Senegal, and Bwamanda, D. R. Congo, respectively. Afework Mulugeta et al. (2010) observed z-score standard deviations of 1.8 (height-for-age), 1.3 (weight-for-age), and 1.3 (weight-for-height) for children in northern Ethiopia. Ephraim Chirwa and Harold Ngalawa (2008) measure z-score standard deviations of 1.321 (WAZ), 1.903 (HAZ), and 1.721 (WHZ) for children across Malawi. Achenef Motbainor et al. (2015) found z-score standard deviations of 1.42 (HAZ), and 1.58 (HAZ) for children in the Gojjam zones of Ethiopia. Bealu Betebo et al. (2017) report standard deviations of 1.46 (WAZ), 2.29 (HAZ), and 1.88 (WHZ) for children in East Badawacho District, Ethiopia with even larger deviations among food insecure households.

In addition, Paul B. Spiegel et al. (2004) performed a meta-analytical quality assessment of anthropometric surveys with no mention of standard deviation. Daniel E. Roth et al. (2017) estimated that across 64 low-and middle-income countries, when mean height-for-age z-scores were zero, the standard deviation was 2.10 (95% CI 2.00 to 2.20), far above most QC thresholds. Examining MUAC (Mid-Upper Arm

Circumference) for 852 cross-sectional nutritional surveys of children, Frison et al. (2016) found that only 319, or 37.7 percent, follow a normal distribution.

In his survey of famines and economics, Martin Ravallion remarks on the unusual nature of malnourished communities: “I will say that a geographic area experiences famine when unusually high mortality risk is associated with an unusually severe threat to the food consumption of at least some people in the area” (1997, p. 1205). The phenomenon of malnutrition is by its very nature unusual i.e., not normal. It would be bizarre to think that measures would behave the same in lean times as in abundance. In their appraisal of different anthropometric indices, André Briand et al. get to the heart of the matter when they observe “for most populations, little information is available on the amount of nutritional change one has to expect in a community and also on the standard deviations of some nutritional indices” (1989, p. 770).

4.6 Peroratio: Eschew the Maxim

The SD as QC maxim is built on a history of shaky citations, corroborated with imprudent tests, substantiated by logical fallacies, and endorsed inconsistently by empiricists. It lacks archival, statistical, logical, theoretical, and practical merit. Of course, there are inaccurate surveys and samples that don’t deserve our consideration, but other tests and conditions must be adopted. The solution to the issue of SD abused as QC is simple: stop doing it. I think of the old vaudeville line of a man who says, “Doctor, it hurts when I do this,” and the doctor replies, “Then don’t do that!”

Having abandoned the SD as QC maxim, the therapeutic and ameliorative next step is more difficult. But good science is difficult. If it were easy, it would have

already been done (Wasserstein et al., 2019). Good science embraces the explicable and ineffable (McCloskey, 1994). Doing serious scientific inquiries calls for serious thinking about what makes a dataset “good” or “bad” and how its “goodness” may impact the results. We need to consider the dozens of sources of *real* error, and reckon their effects on our results. As Ziliak and McCloskey put it, “After all, reconciling differences of effect, finding the common ground, is the point of statistics. ... Most important is to minimize Error of the Third Kind, ‘the error of undue inattention’” (2008, p. 246).

5 Environmental and Economic Determinants of Malnutrition: A Quantitative Spatially Explicit Hierarchical Analysis of Children in Kenya and Nigeria

5.1 Overview

Despite a remarkable reduction in global poverty and famines, substantial childhood malnutrition continues to persist. In 2017, acute malnutrition (*wasting*) menaced over 50 million young children while over 150 million young children suffered from chronic malnutrition (*stunting*). Yet the quantifiable impacts of many determinants are obscure. I have combined health and demographic data from Kenya and Nigeria Demographic Health Surveys (2003, 2008-09, 2013, 2014) with spatially explicit precipitation, temperature, and vegetation data. Using four-level random intercept hierarchical generalized logit models, I evaluated the responsiveness of malnutrition indicators. I found spatial and hierarchical relationships explain 28-36 percent of malnutrition outcome variation. Changes in precipitation, temperature, or vegetation alone can move malnutrition rates by more than 50%. Wasting is most impacted by mother's education, family wealth, clinical delivery, and vaccinations. Stunting is most impacted by family wealth, mother's education, clinical delivery, vaccinations, and children asymptomatic of fever, cough, or diarrhea. Geospatial and disaggregated data helps to understand better who is at risk and where to target mitigation efforts. Remotely monitored climatic variables are powerful determinants, however, their effects vary across different indicators and locations.

5.2 Introduction

Childhood malnutrition is a pernicious public health issue. Malnutrition is a detrimental and significant plight for young children; it is responsible for 45% of all deaths among children worldwide (Black et al., 2013). Malnutrition not only increases child morbidity and mortality, it also inhibits cognitive, social, and financial potential (de Onis & Branca, 2016; Smith & Haddad, 2000). Progress to reduce malnutrition so far is insufficient to attain the World Health Assembly targets for 2025 and the Sustainable Development Goals for 2030 (i.e., a 40% reduction in stunting prevalence and reduce wasting prevalence to less than 5% by 2025, and by 2030 end all forms of malnutrition) (Nations, 2015; WHO, 2014). Despite downward global trends in undernutrition, only 26 of 202 countries are on track to meet the target (Tzioumis & Adair, 2014; UNICEF, 2018).

Causes of child malnutrition are broadly divided into two etiological categories: illness-related or non-illness-related (Mehta et al., 2013). The focus of this essay is to evaluate the latent determinants that impact the severity and variability of non-illness-related childhood malnutrition. Non-illness-related malnutrition stems from economic, social, environmental, political, or cultural factors that decrease nutrient intake and negatively affect growth and development. The severity of malnutrition is measured by deterioration in key anthropometric indicators.

The two most widely studied indicators are *wasting* and *stunting*. Wasting indicates a deficit in tissue and fat mass, either from weight loss or inability to gain weight. A child, aged 0 to 59 months, is defined as wasted if their weight-for-height is below negative two standard deviations from the median of the WHO Child Growth

Standards (UNICEF, 2013). Stunting indicates impeded skeletal growth. It is a measure of linear growth, representing chronic malnutrition accumulated over time. A child, aged 0 to 59 months, is defined as stunted if their height-for-age is below negative two standard deviations from the median of the World Health Organization Child Growth Standards (UNICEF, 2013).

5.3 Background

Since the introduction of the 1990 UNICEF conceptual framework there has been an upsurge in studies attempting to corroborate it with empirical evidence, driven in part by demand from various aid agencies to understand the drivers of malnutrition in order to better carry out their missions (for more on the state of child malnutrition conceptual frameworks, see section 7.3.1). The conceptual framework models child malnutrition as a hierarchical system (UNICEF, 2020). The hierarchical strata include *immediate*, *underlying*, and *basic* classifications, which some interpretations equate to individual, household, and societal levels, whereby factors at one level influence other levels (UNICEF, 1998). While other reinterpretations focus on distinguishing between *proximal* and *distal* determinants (Buisman et al., 2019).

In their extensive report on the aggregate cross-county determinants of malnutrition Smith and Haddad (2000) identify specific sub-categories of the UNICEF framework. They specify dietary intake and health status as the immediate determinants, which are influenced by the underlying determinants of food security (per capita national food availability), care for mothers and children (women's education and women's status relative to men's), and health environment quality (safe water access), which are in turn influenced by the basic determinants of economic

resource availabilities (per capita national income) and the political environment (democracy score).

Despite long observed environmental effects (e.g., Habicht et al., 1974), and widely anticipated links between climate change and child malnutrition, evidence for the nature of the relationship is just beginning to emerge across expansive spatial and temporal scales (Niles et al., 2020). Others have found that much of the evidence for the impact of climate on childhood malnutrition is based on a few heterogeneous studies with flawed methodologies (Phalkey et al., 2015).

Indeed, I find numerous opportunities in the literature for studies, with more geographic coverage, and more attention to scale, that include multiple dimensions of nutrition outcomes, and are couched in sound inferential theory to quantify the spatial, social, political, climatic, and economic determinants of malnutrition. I aim to avail myself of these opportunities by quantifying non-illness-related determinants of stunting and wasting across Kenya and Nigeria through a spatially explicit hierarchical modeling approach consistent with the UNICEF (1990, 1998) conceptual framework.

5.4 Methods

There are over 4.8 million wasted children and over 10 million stunted children in Nigeria, while there are over 278 thousand wasted children and over 1.8 million stunted children in Kenya (UNICEF, 2013). Globally Nigeria has the second highest number of stunted children behind India. Kenya provides a measure of external validity to the analysis and adds variability in terms of malnutrition prevalence rates, governance, climate, population, economy and culture.

To supply the primary data on child health and household characteristics, I employ the Demographic and Health Surveys (DHS) Kids Recode files and the Geographic Data files for Kenya and Nigeria of DHS-IV (1997 to 2003), DHS-V (2003 to 2008), DHS-VI (2008 to 2013) (see Table 3, Table 4, and Table 5). The sample includes 48,086 Nigerian children and 28,421 Kenyan children. To understand each variable and its contents, Measure DHS (2008, 2012, 2013) provides descriptions of the recode data-files and methodologies in a standardized manual (for more on the study design and sample methodology, see section 7.3.2).

I construct the z-scores of anthropometric indices using Stata Statistical Software (Leroy, 2011; StataCorp, 2017). I input the *weight* and *height* measurements along with the *sex* and *child's age* to calculate z-scores in accordance with the 2006 World Health Organization growth standards (UNICEF, 2013). The Nigerian sample includes 7,361 (15.3% prevalence) *wasted* and 18,723 (38.9% prevalence) *stunted* children. The Kenyan sample includes 1,775 (6.3% prevalence) *wasted* and 8,396 (29.5% prevalence) *stunted* children. The dependent variables, *wasting* and *stunting*, are child-level composite binary indicators equal to one if the child's calculated z-score is below negative two standard deviations from the reference median and zero otherwise.

Unique identifiers link the georeferenced data to records in the household surveys at the cluster level. However, the Demographic and Health Surveys employ geographic-masking with a coordinate displacement process to protect respondent confidentiality. The process displaces urban clusters up to two kilometers, displaces rural clusters up to five kilometers, and randomly selects one percent of the rural

clusters to displace up to ten kilometers (Burgert et al., 2013). I link the Kids Recode files via timestamps and the cluster-level spatial identifiers to remotely monitored climatic variables (for more on composition of variables, see section 7.3.3).

Selected covariates follow the UNICEF (1998) conceptual framework along with spatially explicit temperature (CHIRTS), precipitation (CHIRPS), NDVI (Normalized Difference Vegetation Index) and anomaly climatic inputs (Funk et al., 2015; Vermote et al., 2014). The conceptual framework models child malnutrition as a hierarchical system. The multisectoral framework encompasses food, health, and caring practices to help identify the most appropriate mixture of actions. The emphasis of the model is on accommodating many possible determinants of malnutrition and prioritizing the most important within a specific contextual application while being easy to communicate across different users (UNICEF, 1990). Summary statistics of discrete variables, continuous variables, and the hierarchical decomposition are presented in Table 3, Table 4, and Table 5 respectively.

Table 3. Summary statistics of discrete variables

<i>Variable</i>	Nigeria		Kenya	
	<i>Frequency</i>	<i>Percent</i>	<i>Frequency</i>	<i>Percent</i>
Wasting Status				
Not wasted	40,716	84.69	26,646	93.75
Wasted	7,360	15.31	1,775	6.25
Stunting Status				
Not stunted	29,353	61.06	20,025	70.46
Stunted	18,723	38.94	8,396	29.54
Sex				
Male	23,991	49.90	14,369	50.56
Female	24,085	50.10	14,052	49.44
Delivery				
Home	29,850	62.38	14,069	49.63
Clinic	18,002	37.62	14,277	50.37
Birth				
Multiple	1,428	2.97	734	2.58
Singleton	46,648	97.03	27,687	97.42
Weaned				
Breastfed beyond 1 year	16,809	34.96	7,158	25.19
Weaned by 1 year	19,645	40.86	14,896	52.41
Breastfed up to 1 year	11,038	22.96	4,170	14.67
Weaned before 1 year	584	1.21	2,197	7.73
Vaccines - Minimum				
No	12,181	25.36	1,341	4.72
Yes	35,850	74.64	27,073	95.28
Vaccines - Maximum				
No	40,684	84.70	16,965	59.71
Yes	7,347	15.30	11,449	40.29
Diet				
Unvaried	35,622	74.10	22,723	79.95
Diverse	12,454	25.90	5,698	20.05
Sick				
Symptomatic	12,709	26.66	14,226	50.14
Asymptomatic	34,957	73.34	14,149	49.86
Latrine - Improved				
No	32,967	70.96	22,184	82.36
Yes	13,489	29.04	4,751	17.64
Water - Improved				
No	22,082	47.07	11,540	41.37
Yes	24,833	52.93	16,355	58.63
Residence				
Urban	15,680	32.62	8,179	28.78
Rural	32,396	67.38	20,242	71.22
Mothers Education				
None	21,919	45.59	5,992	21.08
Primary	10,898	22.67	15,521	54.61
Secondary	12,471	25.94	5,280	18.58
Higher	2,788	5.80	1,628	5.73
Wealth Index				
Poorest	10,697	22.25	9,077	31.94
Poorer	10,813	22.49	5,784	20.35
Middle	9,678	20.13	4,856	17.09
Richer	9,035	18.79	4,333	15.25
Richest	7,853	16.33	4,371	15.38

Table 3. (continued)

<i>Variable</i>	Nigeria		Kenya	
	<i>Frequency</i>	<i>Percent</i>	<i>Variable</i>	<i>Percent</i>
Interview Month				
January	0	0.00	1,530	5.38
February	1,370	2.85	1,265	4.45
March	7,315	15.22	25	0.09
April	8,166	16.99	729	2.57
May	8,709	18.12	4,042	14.22
June	3,932	8.18	4,718	16.60
July	6,327	13.16	4,828	16.99
August	5,698	11.85	4,035	14.20
September	4,043	8.41	4,163	14.65
October	2,485	5.17	805	2.83
November	31	0.06	1,145	4.03
December	0	0.00	1,136	4.00
Survey Phase				
DHS-IV	4,386	9.12	4,718	16.60
DHS-V	19,246	40.02	5,101	17.95
DHS-VI	24,454	50.85	18,602	65.45

Table 4. Summary statistics of continuous variables

<i>Variable</i>	Nigeria				Kenya			
	<i>Average</i>	<i>Standard</i>		<i>Max</i>	<i>Average</i>	<i>Standard</i>		<i>Max</i>
		<i>Deviation</i>	<i>Min</i>			<i>Deviation</i>	<i>Min</i>	
Child's Age (Months)	28.3	17.2	0	59	28.9	17	0	59
Mother's Age (Years)	29.5	6.93	15	49	28.6	6.57	15	49
Birth Tally	4.3	2.58	1	18	3.8	2.36	1	16
Precipitation (dm)	21.3	7.95	4.7	61.6	8.3	6.13	0.02	25.2
Temperature (°C)	31	2.23	24	38.3	26.4	3.7	15.6	35.6
Precipitation Anomaly	0.2	2.62	-11.3	11.4	-0.5	1.47	-5.5	8.2
Temperature Anomaly	-0.7	0.46	-1.9	0.7	-0.8	0.45	-2.6	0.9
NDVI	0.6	0.14	0.09	0.9	0.6	0.14	0.1	0.9
NDVI Anomaly	0.0	0.026	-0.1	0.2	0.0	0.034	-0.1	0.2

Table 5. Hierarchical decomposition of DHS

<i>Scale</i>	Nigeria				Kenya			
	<i>Groups</i>	<i>Observations per Group</i>			<i>Groups</i>	<i>Observations per Group</i>		
		<i>Min</i>	<i>Average</i>	<i>Max</i>		<i>Min</i>	<i>Average</i>	<i>Max</i>
State	37	765	1,299.1	2,750	47	339	600.9	1,165
Cluster	2,131	1	22.6	79	2,365	1	11.9	43
Household	30,904	1	1.6	8	20,048	1	1.4	6
Child	48,068				28,241			

5.5 Analysis

All results and conclusions are drawn from a *four-level random intercept hierarchical generalized logit model* (for the specification, see section 7.3.5.6). However, I performed preliminary supplementary analyses, too, including a *linear probability* specification and *logit* specification (for their specifications, see sections 7.3.5.2 and 7.3.5.4). Given the discrete nature of the dependent variables, wasting and stunting, I use linear probability and logit models to motivate the initial coefficient interpretations and provide a lower bound on effect sizes (for ancillary results tables, see section 7.3.9). Exploring multiple model specifications helps to minimize specification error and maximizes validity (for more on the econometric motivation, see sections 7.3.5.1 and 7.3.5.5). Utilizing different populations gives protection against confounding (Smith & Ebrahim, 2002).

5.6 Results

Prevalence rates of wasting and stunting are overall spatially correlated, although there are pockets where rates deviate substantially suggesting different causal pathways (Figure 6). The variable heterogeneity of malnutrition prevalence over the landscape highlights the need for a disaggregate and spatially explicit modeling approach (for more detailed spatial distributions and uncertainty estimates, see section 7.3.4158). The Demographic and Health Surveys data form a natural hierarchical structure: regions within a country, states within a region, clusters within a state, households within a cluster, occupants within a household, and children for each woman.

The results across the various modeling approaches tell a consistent story, implying the results are robust to particular modeling variations. My results indicate that the hierarchical structure alone explains 28 to 36 percent of the variation in malnutrition, meaning the additional model complexity has consequential explanatory value (Table 6).

Table 6. Unconditional Hierarchical Model - Variance Decomposition

Hierarchical Fully Unconditional	Wasted		Stunted	
	Nigeria	Kenya	Nigeria	Kenya
<i>Variance Decomposition – Percent by Level</i>				
States	7.09%	11.35%	10.94%	1.87%
Clusters	9.48%	6.35%	6.99%	6.11%
Households	17.50%	20.09%	13.31%	20.08%
Children	65.93%	62.22%	68.77%	71.94%

The results of the discrete covariates illustrate how much each categorical determinant affects malnutrition for a change from a baseline counterfactual (Figure 7). As a general rule the effect sizes for stunting are larger than for wasting due to the smaller prevalence of wasting in the population. Because the model results measure the direct impact on the percentage point difference in probability of malnutrition in the population (i.e., prevalence), the size of the marginal effects have an upper-bound limit of the prevalence in the population (for more on interpretation of results, see section 7.3.6). In other words, only already wasted and stunted children can transition to being non-wasted and non-stunted.

In both Nigeria and Kenya, mother’s education plays a greater role in determining wasting, whereas household wealth is the leading determinant of stunting. On average, in Nigeria, the probability of being wasted is 4 percentage points (95% CI: -5.4 to -2.7) lower for a child from a mother with higher education

than from a mother with no education (Table 7). Whereas in Kenya, the probability of being wasted is 1.7 percentage points (95% CI: -2.6 to -0.89) lower for a child from a mother with higher education than from a mother with no education (Table 7). That is to say, the absolute prevalence rates of wasting in Nigeria and Kenya would drop from 15.31% and 6.25% respectively down to 11.31% and 4.55% if mothers of wasted children had higher education holding all else constant. Education plays a vital role for reducing stunting prevalence, too. Mothers attaining higher education can reduce stunting rates by 13 percentage points (95% CI: -16 to -10) in Nigeria and 5.9 percentage points (95% CI: -10 to -1.3) in Kenya (Table 8). In other words, education alone has the potential to curtail the number of stunted children by over one third.

In terms of quantifying the results in numbers of children, and in numbers of deaths prevented, the effects are highly epidemiologically significant. In 2011, the Nigeria under-five population was 27,195,000 with a 41% stunting prevalence (11,149,950) and a 14% wasting prevalence (3,807,300), with an overall mortality rate of 124/1000 for under-fives (3,372,180), which is much lower for non-malnourished children making the deaths prevented estimates conservative lower bounds of their true values (UNICEF, 2013). Using a maximally adjusted, minimum hazard ratio, of 2.12 for stunting mortality and 3.47 for wasting mortality, the mortality rate becomes 260/1000 at a minimum for stunted children, and 430/1000 at a minimum for wasted children (Olofin et al., 2013). If at a maximum education can reduce stunting prevalence by 13 percentage points, or by 3,353,350 children, then education can prevent at a minimum approximately 490,989 children's deaths.

Similarly, if at a maximum education can reduce wasting prevalence by 4 percentage points, or by 1,087,800 children, then education can prevent at a minimum approximately 315,767 children's deaths. In 2011, the Kenya under-five population was 6,805,000 with a 35% stunting prevalence (2,381,750) and a 7% wasting prevalence (476,350), with an overall mortality rate of 73/1000 for under-fives (496,765), which is much lower for non-malnourished children making the deaths prevented estimates conservative lower bounds of their true values (UNICEF, 2013). Using a maximally adjusted, minimum hazard ratio, of 2.12 for stunting mortality and 3.47 for wasting mortality, the mortality rate becomes 150/1000 at a minimum for stunted children and 250/1000 at a minimum for wasted children (Olofin et al., 2013). If at a maximum education can reduce stunting prevalence by 5.9 percentage points, or by 401,495 children, then education can prevent at a minimum approximately 32,826 children's deaths. Similarly, if at a maximum education can reduce wasting prevalence by 1.7 percentage points, or by 115,685 children, then education can prevent at a minimum approximately 21,206 children's deaths.

Wealth also has a powerful influence on malnutrition rates. The richest families from the highest wealth quintile can reduce wasting prevalence by 0.95 percentage points (95% CI: -2.5 to 0.63) in Nigeria and 1.2 percentage points (95% CI: -2.3 to -0.17) in Kenya, and can reduce stunting prevalence by 16 percentage points (95% CI: -18 to -13 and -19 to -12) in both Nigeria and Kenya (Table 7 and Table 8). The richer or second highest wealth quintile can reduce wasting prevalence by 1.6 percentage points (95% CI: -2.8 to -0.42) in Nigeria and 1.1 percentage points (95% CI: -1.8 to -0.3) in Kenya, and can reduce stunting prevalence by 12 percentage

points (95% CI: -15 to -9.9) in Nigeria and 10 percentage points (95% CI: -13 to -6.9) in Kenya (Table 7 and Table 8). Moving to the middle wealth quintile reduces wasting prevalence by 1.3 percentage points (95% CI: -2.2 to -0.45) in Nigeria and 0.79 percentage points (95% CI: -1.5 to -0.04) in Kenya, and reduces stunting prevalence by 6 percentage points (95% CI: -8.2 to -3.9) in Nigeria and 8.1 percentage points (95% CI: -11 to -5.4) in Kenya (Table 7 and Table 8). Even moving from the poorest to second poorest wealth quintile can reduce wasting prevalence by 0.06 percentage points (95% CI: -0.92 to 0.8) in Nigeria and 0.92 percentage points (95% CI: -1.6 to -0.22) in Kenya, and can reduce stunting prevalence by 2.9 percentage points (95% CI: -4.7 to -1.1) in Nigeria and 4.3 percentage points (95% CI: -6.7 to -1.9) in Kenya (Table 7 and Table 8). Overall, changes in wealth alone have a smaller but substantial impact on wasting with reductions up to one fifth. Even more substantially, wealth alone has the potential to curtail the number of stunted children by more than one half.

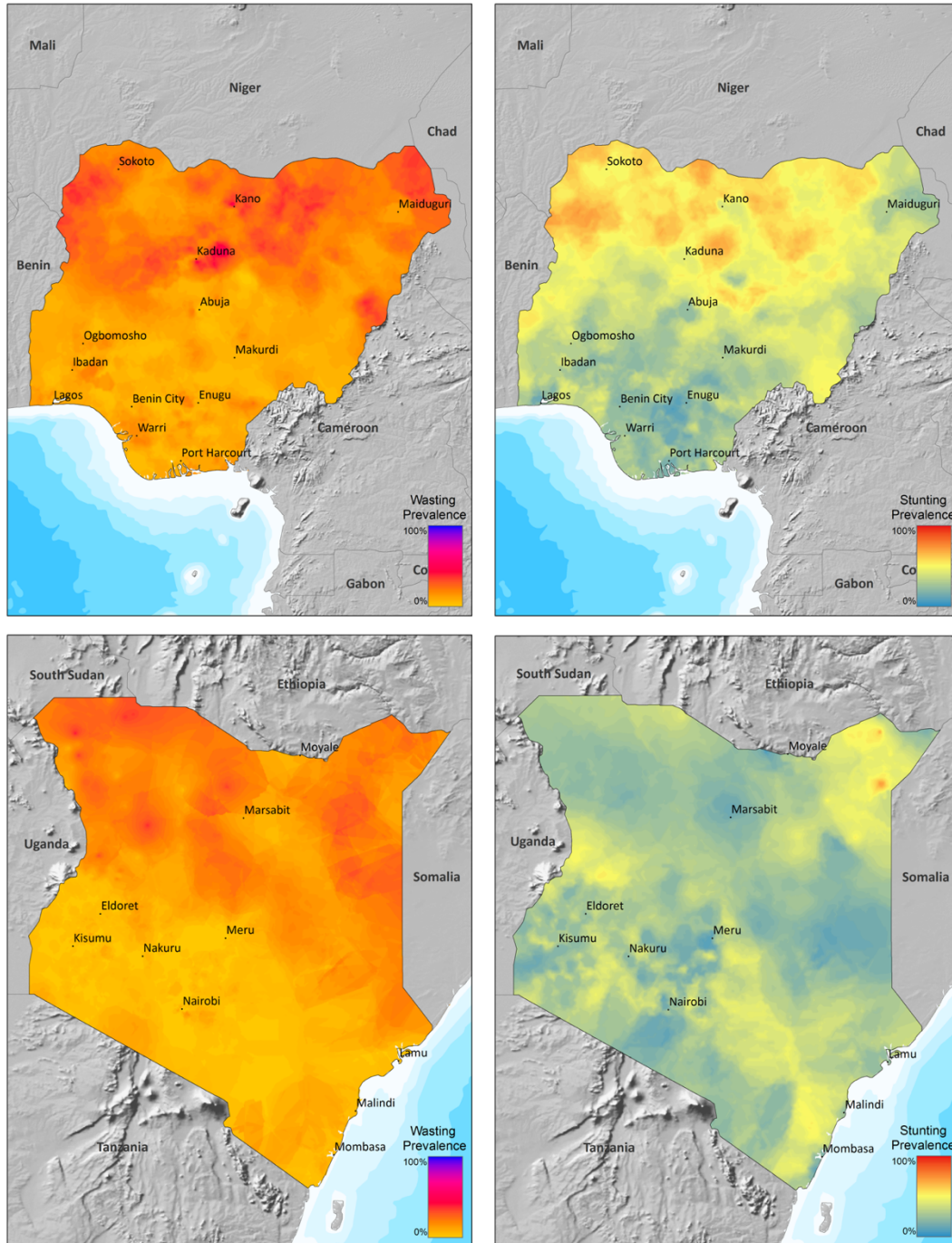


Figure 6: Empirical Bayesian kriging of sample malnutrition prevalence across Kenya and Nigeria DHS-IV, DHS-V, and DHS-VI using ArcGIS software by ESRI (2017). Color gradients indicate prevalence of stunting and wasting malnutrition rates.

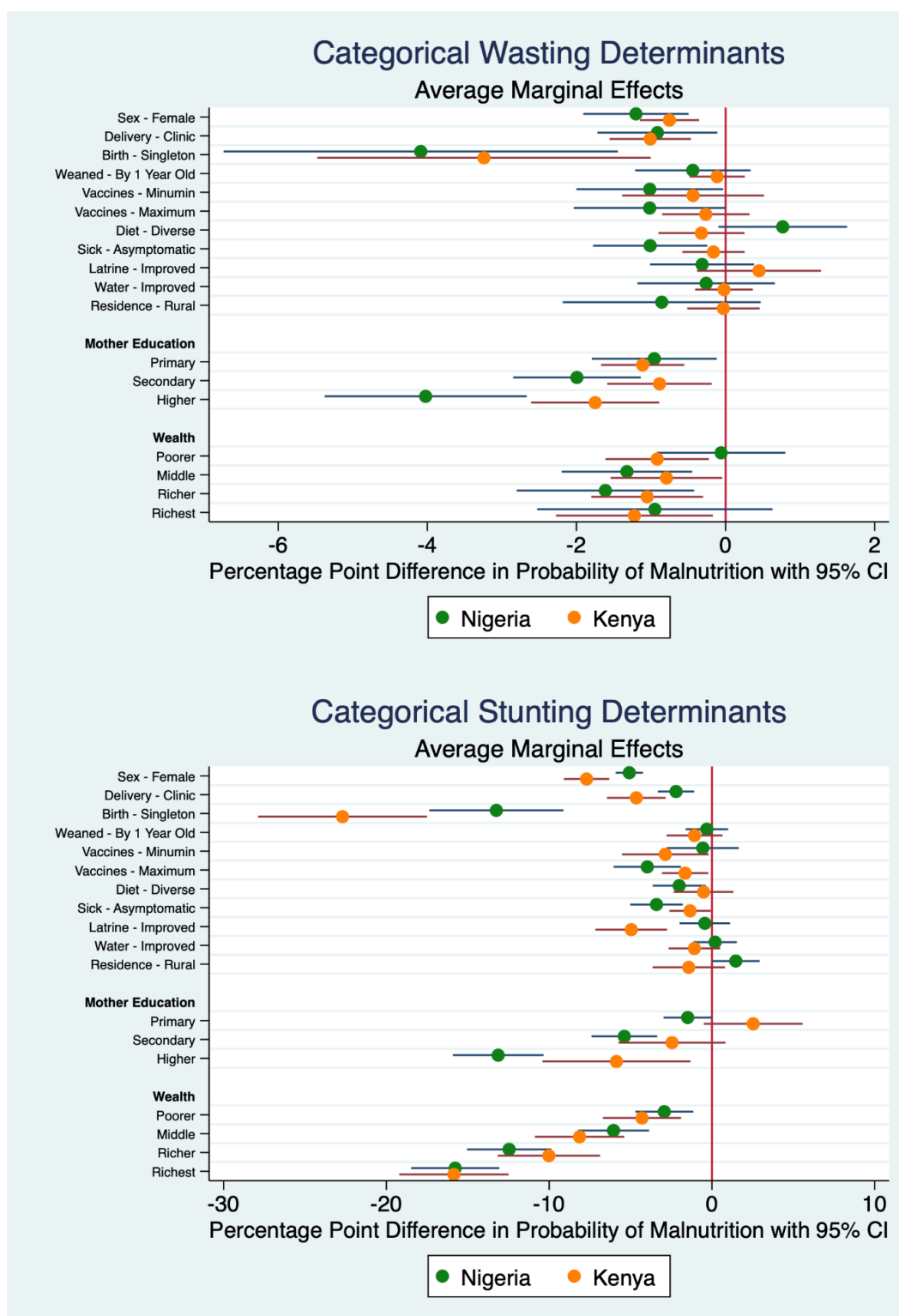


Figure 7: Average marginal effects of categorical determinants of malnutrition (based on Table 7 and Table 8). Variables are displayed such that negative values are beneficial for children's health and positive values are deleterious for children's health. The vertical red line at zero demarks the liminal threshold, whereas the green and orange horizontal lines are 95% confidence intervals.

Table 7. Interpreted Hierarchical Analyses, Wasted Percentage Point Change

Interpreted Results Hierarchical Random Intercept	Percent Change in Wasted Probability Nigeria				Kenya
<i>For a Change from Baseline Category with 95% Confidence Interval in Brackets</i>					
Sex - Female	-1.2%	[-1.9, -0.49]	-0.75%		[-1.1, -0.36]
Delivery - Clinic	-0.91%	[-1.7, -0.11]	-1%		[-1.6, -0.46]
Birth - Singleton	-4.1%	[-6.7, -1.4]	-3.2%		[-5.5, -1]
Weaned - By 1 Year Old	-0.44%	[-1.2, 0.34]	-0.11%		[-0.48, 0.26]
Vaccines - Minimum	-1%	[-2, -0.03]	-0.44%		[-1.4, 0.52]
Vaccines - Maximum	-1%	[-2, 0]	-0.27%		[-0.85, 0.32]
Diet - Diverse	0.77%	[-0.1, 1.6]	-0.32%		[-0.9, 0.25]
Sick - Asymptomatic	-1%	[-1.8, -0.25]	-0.16%		[-0.58, 0.26]
Latrine - Improved	-0.31%	[-1, 0.38]	0.45%		[-0.38, 1.3]
Water - Improved	-0.26%	[-1.2, 0.66]	-0.02%		[-0.41, 0.37]
Residence - Rural	-0.86%	[-2.2, 0.47]	-0.03%		[-0.51, 0.46]
Mothers Education					
Primary	-0.96%	[-1.8, -0.12]	-1.1%		[-1.7, -0.56]
Secondary	-2%	[-2.8, -1.1]	-0.89%		[-1.6, -0.18]
Higher	-4%	[-5.4, -2.7]	-1.7%		[-2.6, -0.89]
Wealth Index					
Poorer	-0.06%	[-0.92, 0.8]	-0.92%		[-1.6, -0.22]
Middle	-1.3%	[-2.2, -0.45]	-0.79%		[-1.5, -0.04]
Richer	-1.6%	[-2.8, -0.42]	-1.1%		[-1.8, -0.3]
Richest	-0.95%	[-2.5, 0.63]	-1.2%		[-2.3, -0.17]
<i>For a 1-Unit Increase in Determinant with 95% Confidence Interval in Brackets</i>					
Child's Age	-2.2%	[-2.8, -1.5]	-0.13%		[-0.38, 0.12]
Mother's Age	0.26%	[-0.64, 1.2]	-0.22%		[-0.65, 0.2]
Birth Tally	-0.17%	[-0.39, 0.05]	0.07%		[-0.07, 0.21]
Precipitation	-0.96%	[-2.3, 0.41]	-1.5%		[-2.5, -0.63]
Temperature	1.2%	[0.79, 1.5]	0.24%		[0.12, 0.36]
Precipitation Anomaly	-0.45%	[-4.9, 4]	1.1%		[-0.88, 3.1]
Temperature Anomaly	-2.7%	[-5.2, -0.26]	-0.01%		[-0.62, 0.61]
NDVI	-9.2%	[-14, -4.9]	-3.9%		[-6.6, -1.3]
NDVI Anomaly	4.4%	[-14, 23]	5.5%		[-2.1, 13]
<i>For a 1- Standard Deviation Increase in Determinant with 95% Confidence Interval in Brackets</i>					
Child's Age	-3.15%	[-4.01, -2.15]	-0.18%		[-0.54, 0.17]
Mother's Age	0.18%	[-0.44, 0.83]	-0.14%		[-0.43, 0.13]
Birth Tally	-0.44%	[-1.01, 0.14]	0.16%		[-0.17, 0.5]
Precipitation	-0.76%	[-1.83, 0.33]	-0.92%		[-1.53, -0.39]
Temperature	2.68%	[1.76, 3.35]	0.89%		[0.44, 1.33]
Precipitation Anomaly	-0.12%	[-1.28, 1.05]	0.16%		[-0.13, 0.46]
Temperature Anomaly	-1.24%	[-2.39, -0.12]	0%		[-0.28, 0.27]
NDVI	-1.29%	[-1.96, -0.69]	-0.55%		[-0.92, -0.18]
NDVI Anomaly	0.11%	[-0.36, 0.6]	0.19%		[-0.07, 0.44]
<i>For a Sample Maximum Increase in Determinant with 95% Confidence Interval in Brackets</i>					
Child's Age	-10.82%	[-13.77, -7.38]	-0.64%		[-1.87, 0.59]
Mother's Age	0.88%	[-2.18, 4.08]	-0.75%		[-2.21, 0.68]
Birth Tally	-2.89%	[-6.63, 0.92]	1.04%		[-1.08, 3.15]
Precipitation	-5.46%	[-13.09, 2.33]	-3.78%		[-6.3, -1.59]
Temperature	17.16%	[11.3, 21.45]	4.8%		[2.4, 7.2]
Precipitation Anomaly	-1.02%	[-11.12, 9.08]	1.51%		[-1.21, 4.25]
Temperature Anomaly	-7.02%	[-13.52, -0.68]	-0.02%		[-2.17, 2.14]
NDVI	-7.45%	[-11.34, -3.97]	-3.12%		[-5.28, -1.04]
NDVI Anomaly	1.32%	[-4.2, 6.9]	1.65%		[-0.63, 3.9]

Table 8. Interpreted Hierarchical Analyses, Stunted Percentage Point Change

Interpreted Results Hierarchical Random Intercept	Percent Change in Stunted Probability			
	Nigeria		Kenya	
<i>For a Change from Baseline Category with 95% Confidence Interval in Brackets</i>				
Sex - Female	-5.1%	[-5.9, -4.2]	-7.7%	[-9.1, -6.3]
Delivery - Clinic	-2.2%	[-3.3, -1.1]	-4.6%	[-6.4, -2.8]
Birth - Singleton	-13%	[-17, -9.1]	-23%	[-28, -18]
Weaned - By 1 Year Old	-0.31%	[-1.6, 1]	-1.1%	[-2.8, 0.65]
Vaccines - Minimum	-0.56%	[-2.8, 1.7]	-2.9%	[-5.5, -0.2]
Vaccines - Maximum	-4%	[-6, -1.9]	-1.6%	[-3.1, -0.22]
Diet - Diverse	-2%	[-3.6, -0.37]	-0.51%	[-2.3, 1.3]
Sick - Asymptomatic	-3.4%	[-5, -1.8]	-1.3%	[-2.6, -0.07]
Latrine - Improved	-0.43%	[-2, 1.1]	-5%	[-7.2, -2.8]
Water - Improved	0.2%	[-1.1, 1.5]	-1.1%	[-2.7, 0.51]
Residence - Rural	1.5%	[0.01, 2.9]	-1.4%	[-3.6, 0.8]
Mothers Education				
Primary	-1.5%	[-3, -0.01]	2.5%	[-0.5, 5.6]
Secondary	-5.4%	[-7.4, -3.4]	-2.5%	[-5.7, 0.84]
Higher	-13%	[-16, -10]	-5.9%	[-10, -1.3]
Wealth Index				
Poorer	-2.9%	[-4.7, -1.1]	-4.3%	[-6.7, -1.9]
Middle	-6%	[-8.2, -3.9]	-8.1%	[-11, -5.4]
Richer	-12%	[-15, -9.9]	-10%	[-13, -6.9]
Richest	-16%	[-18, -13]	-16%	[-19, -12]
<i>For a 1-Unit Increase in Determinant with 95% Confidence Interval in Brackets</i>				
Child's Age	-0.75%	[-1.7, 0.16]	-2.6%	[-3.3, -1.8]
Mother's Age	-3.6%	[-4.7, -2.5]	-4.6%	[-6.1, -3.1]
Birth Tally	0.37%	[0.06, 0.68]	1.1%	[0.67, 1.6]
Precipitation	-1.5%	[-4.4, 1.4]	3.3%	[0.33, 6.3]
Temperature	-0.26%	[-1.3, 0.73]	-0.92%	[-1.2, -0.61]
Precipitation Anomaly	5.2%	[-1, 11]	-3.4%	[-8, 1.2]
Temperature Anomaly	-1.8%	[-5.8, 2.1]	1%	[-0.43, 2.4]
NDVI	-6.6%	[-19, 6.1]	12%	[5.7, 18]
NDVI Anomaly	30%	[-20, 80]	-13%	[-36, 9.8]
<i>For a 1- Standard Deviation Increase in Determinant with 95% Confidence Interval in Brackets</i>				
Child's Age	-1.08%	[-2.44, 0.23]	-3.68%	[-4.68, -2.55]
Mother's Age	-2.49%	[-3.26, -1.73]	-3.02%	[-4.01, -2.04]
Birth Tally	0.95%	[0.16, 1.75]	2.6%	[1.58, 3.78]
Precipitation	-1.19%	[-3.5, 1.11]	2.02%	[0.2, 3.86]
Temperature	-0.58%	[-2.9, 1.63]	-3.4%	[-4.44, -2.26]
Precipitation Anomaly	1.36%	[-0.26, 2.88]	-0.5%	[-1.18, 0.18]
Temperature Anomaly	-0.83%	[-2.67, 0.97]	0.45%	[-0.19, 1.08]
NDVI	-0.92%	[-2.66, 0.85]	1.68%	[0.8, 2.52]
NDVI Anomaly	0.78%	[-0.52, 2.08]	-0.44%	[-1.22, 0.33]
<i>For a Sample Maximum Increase in Determinant with 95% Confidence Interval in Brackets</i>				
Child's Age	-3.69%	[-8.36, 0.79]	-12.78%	[-16.23, -8.85]
Mother's Age	-12.24%	[-15.98, -8.5]	-15.64%	[-20.74, -10.54]
Birth Tally	6.29%	[1.05, 11.56]	16.5%	[10.05, 24]
Precipitation	-8.54%	[-25.04, 7.97]	8.31%	[0.83, 15.86]
Temperature	-3.72%	[-18.59, 10.44]	-18.4%	[-24, -12.2]
Precipitation Anomaly	11.8%	[-2.27, 24.97]	-4.66%	[-10.96, 1.64]
Temperature Anomaly	-4.68%	[-15.08, 5.46]	3.5%	[-1.51, 8.4]
NDVI	-5.35%	[-15.39, 4.94]	9.6%	[4.56, 14.4]
NDVI Anomaly	9%	[-6, 24]	-3.9%	[-10.8, 2.94]

The climate variables are a remote monitoring corollary to malnutrition. Climate variables have the potential to act as leading indicators for changes in malnutrition prevalence with wide coverage and lower costs as compared to traditional clinical survey techniques. Malnutrition is often purported to be the most significant impact of climate change on children's health, but little empirical evidence exists in the literature (Grace et al., 2014; Phalkey et al., 2015; Shively, 2017). For Nigeria and Kenya, NDVI, precipitation, and temperature levels all play a significant, if not homogeneous, role in determining wasting and stunting prevalence.

The effect of temperature on Nigeria wasting shows that higher temperatures correspond to higher wasting prevalence, and on average a maximum monthly temperature of 38°C in the preceding growing season is associated with a 25% wasting prevalence (Figure 9). That is to say, higher temperature corresponds to a 10 percentage points higher wasting prevalence. In a forecasting regime, the model results show that if temperatures in Nigeria reach an average monthly maximum of 38°C during the growing season, then the following year one in four children will experience wasting. Similarly, if temperatures in Kenya reach an average monthly maximum of 35°C during the growing season, then the following year one in ten children will experience wasting: a nearly two-fold increase from the observed prevalence (Figure 9).

Increases in precipitation levels in the preceding growing season can have an ameliorative effect. If precipitation levels in Kenya reach 2.5 dm over the growing season, then the following year 3% of children may experience wasting, or over a 50% reduction from the sample average (Figure 8). And if precipitation levels in

Nigeria reach 6.0 dm over the growing season, then the following year one in ten children may experience (Figure 8). NDVI in the preceding growing season is a further measure with a strong inverse relationship that acts to mitigate wasting rates. In both Nigeria and Kenya moving from the lowest to the highest values of observable NDVI would cut wasting rates by 50% (Figure 10).

While the absolute value or level plays the largest and most direct role in determining malnutrition outcomes, the long-term variability or anomaly plays a substantial secondary role, too. One standard deviation increase in precipitation anomaly reduces wasting prevalence by 0.12 percentage points (95% CI: -1.28 to 1.05) in Nigeria and reduces stunting prevalence by 0.5 percentage points (95% CI: -1.18 to 0.18) in Kenya. It increases wasting prevalence by 0.16 percentage points (95% CI: -0.13 to 0.46) in Kenya and increases stunting prevalence by 1.36 percentage points (95% CI: -0.26 to 2.88) in Nigeria (Table 7 and Table 8). One standard deviation increase in temperature anomaly reduces wasting prevalence by 1.24 percentage points (95% CI: -2.39 to -0.12) in Nigeria and has zero effect (95% CI: -0.28 to 0.27) in Kenya. It reduces stunting prevalence by 0.83 percentage points (95% CI: -2.67 to 0.97) in Nigeria, but causes an increase of 0.45 percentage points (95% CI: -0.19 to 1.08) in Kenya (Table 7 and Table 8). One standard deviation increase in NDVI anomaly increases the prevalence of wasting by 0.11 percentage points (95% CI: -0.36 to 0.6) in Nigeria and 0.19 percentage points (95% CI: -0.07 to 0.44) in Kenya. It increases the prevalence of stunting by 0.78 percentage points (95% CI: -0.52 to 2.08) in Nigeria, but causes a decrease of 0.44 percentage points (95% CI: -1.22 to 0.33) in Kenya (Table 7 and Table 8).

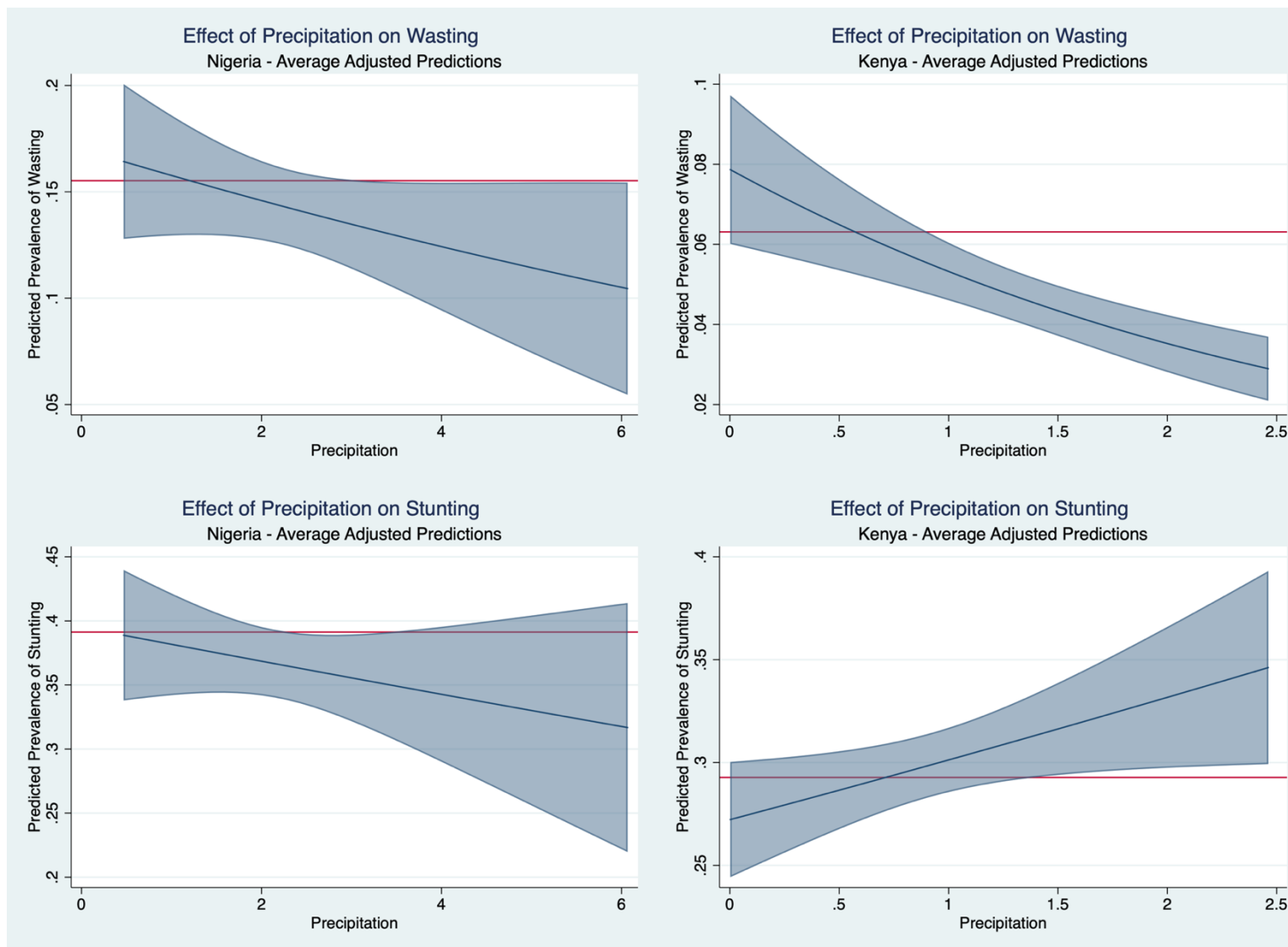


Figure 8: Effect of precipitation on average predicted probability of malnutrition. The horizontal axis is the in-sample range of average total monthly rainfall (dm) during the preceding growing season. The horizontal red line demarks the observed malnutrition prevalence and the sloped blue line illustrates how much the expected prevalence rates change as precipitation changes. The shaded blue corresponds to a 95% confidence interval band on the estimate.

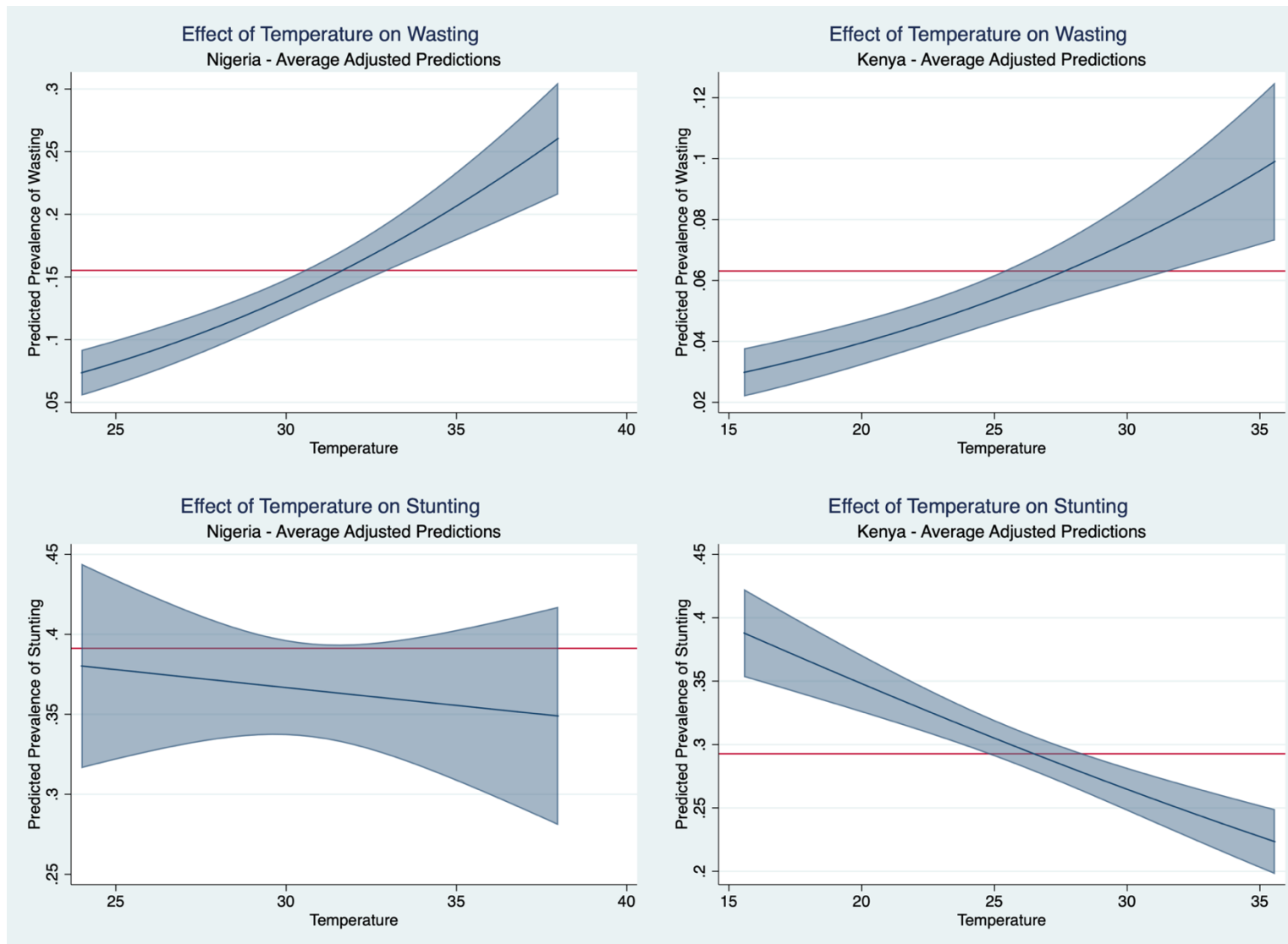


Figure 9: Effect of temperature on average predicted probability of malnutrition. The horizontal axis is the in-sample range of average maximum monthly temperatures ($^{\circ}\text{C}$) during the preceding growing season. The horizontal red line demarks the observed malnutrition prevalence and the sloped blue line illustrates how much the expected prevalence rates change as temperature changes. The shaded blue corresponds to a 95% confidence interval band on the estimate.

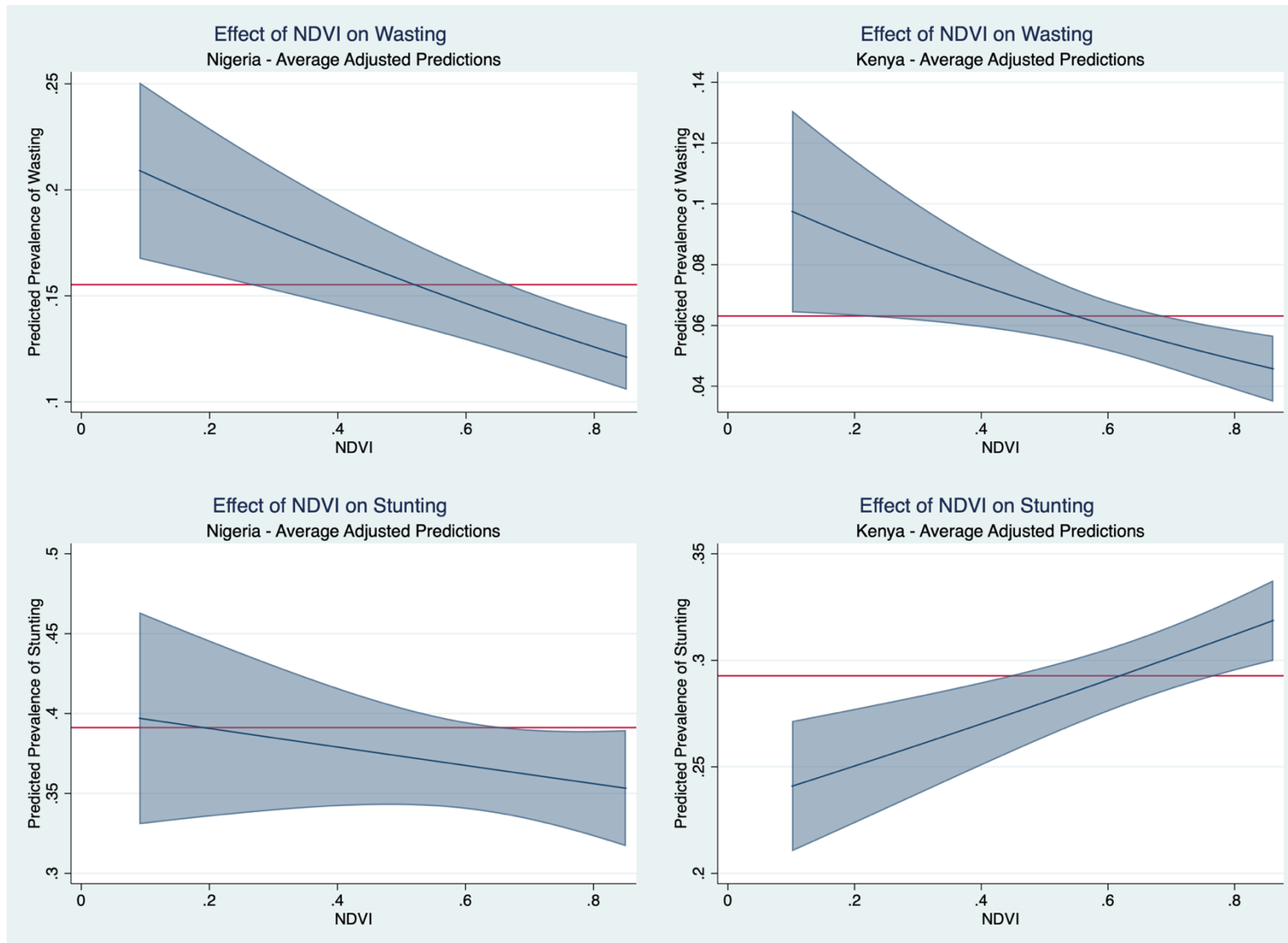


Figure 10: Effect of NDVI on average predicted probability of malnutrition. The horizontal axis is the in-sample range of the unit-less NDVI for the three greenest months during the preceding growing season. The horizontal red line demarks the observed malnutrition prevalence and the sloped blue line illustrates how much the expected prevalence rates change as NDVI changes. The shaded blue corresponds to a 95% confidence interval band on the estimate.

5.6.1 Limitations

Using a standardized questionnaire model, the Demographic and Health Surveys Program aims to collect data that are comparable across countries. However, the questionnaire model has been modified across each of the seven phases of the Program making it difficult to measure changes through time. The DHS geographic displacement process reduces the risk of disclosing confidential personal information, but adds artificial uncertainty into the signal-to-noise ratio and lowers the precision of estimated covariates. In the survey design, individuals within households are not sampled, only clusters are sampled, and then households are sampled within clusters. And, given that the DHS datasets do not provide a separate sampling fraction (i.e., weights) for clusters, households, and individuals for privacy, weighting in a multilevel model is infeasible (DHS, 2008, 2012, 2013). Although the data have a temporal component, successive surveys are repeated cross-sections, not a panel. There remains a need for similar studies that include more countries, across more surveys, across a broader timespan, examining more outcomes with more inputs specifically directed at the nexus of climate, conflict, and malnutrition.

5.6.2 Future Directions

There exists two distinct binary-outcome econometric models of malnutrition each offering valuable insights into the structural design yet they are non-compatible in execution. One is the hierarchical model with random effects for the clustered strata presented here. The other is a misclassification error corrected logit. Given the model specification complexity I did not control for potential misclassification error in the

outcome variable, which may cause attenuated coefficient estimates (Sandler & Rashford, 2018). Exploratory analysis suggests the accuracy of the observed wasted children is as low as 37% (Nigeria) and 21% (Kenya). The accuracy of the observed stunted children is better, 78% (Nigeria) and 66% (Kenya). Unfortunately, even accuracies as high as 95% can still produce wrong and attenuated results leading to incorrect inference about the world. Other estimates for the over dispersion of height-for-age z-scores suggest variance inflation factors as high as 110% (Ghosh et al., 2020). The problem remains of combining the two specifications into a more general flexible model and implementing it in empirical applications.

5.7 Discussion

Malnutrition devastates millions of children every year, yet the latent determinants are largely obscure. Across the African malnutrition literature, there exists a heterogenous patchwork of research which neglects countries where malnutrition is most severe. Furthermore, the disregard for the heterogeneity of social experience, which considers how the determinants change across and between different hierarchies, is questionable. Disaggregate nationally representative empirical studies of childhood malnutrition in Africa are found wanting for answers to two essential questions: *What are* the determinants of malnutrition? And *how much* do the determinants affect malnutrition? Motivated by the hierarchical data structure within a well-established conceptual framework model, this analysis begins to provide much-needed answers.

One is best informed by examining determinants on the basis of their scientific, quantitative, and epidemiological significance. A determinants impact is

most appropriately measured by its ability to change malnutrition prevalence in an epidemiologically significant way. I find the most impactful latent determinants each have the capacity to reduce prevalence rates by as much as 50%: an epidemiologically significant effect.

Although wasting and stunting are related malnutrition indicators their causal pathways, prevalence, duration, impact, and determining factors are distinct. Across both Nigeria and Kenya, stunting is most significantly impacted by family wealth, followed by mother's education, a clinical delivery, vaccinations, and children who are asymptomatic of fever, cough, or diarrhea. In Nigeria diet diversity manifests as a mitigating stunting risk factor, whereas in Kenya access to improved latrine facilities and rural households mitigates stunting prevalence. Wasting is most significantly impacted by mother's education, followed by family wealth, a clinical delivery, and vaccinations across both Nigeria and Kenya. And in Nigeria children living in urban households and those children exhibiting symptoms of fever, cough, or diarrhea are also at elevated wasting risk levels.

Climatic variables are powerful determinants of malnutrition. Across the observable range of values, changes in precipitation, temperature, or NDVI (in the preceding growing season) alone could curtail or inflate the number of wasted and stunted children by more than one half. However, their effects can vary greatly across different nutrition indicators and different countries. Due to the distinct causal pathways and chronic nature of stunting, the signal to noise ratio of climate determinants is markedly diminished. In Kenya higher precipitation and NDVI levels were deleterious and significant determinants, while higher temperature levels were a

mitigating and significant determinant. Yet in Nigeria higher temperature, precipitation, and NDVI levels were mitigating determinants.

Surprisingly, some oft-purported determinants of malnutrition were not significant in both the statistical or epidemiological sense. These include climate anomalies, access to improved latrine facilities, access to improved water facilities, weaning practices, and diet diversity for wasting. Similarly, for stunting, improved water facilities and weaning are not significant. Further research is needed to ameliorate these discrepancies.

The inconsistencies of determinants across space and malnutrition outcomes highlight the need for prudent, highly specific, and tailored approaches, especially when using climate determinants for any forecasting efforts or policy interventions (e.g. Kinyoki, Berkley, et al., 2016a, 2016b). Particular focus should be paid to those determinants that are either actionable by policy intervention or serviceable in forecasting and intervention efforts as well as epidemiologically significant. Identifying effective mitigating determinants to prevent the harmful effects of malnutrition in children should be a priority. Only with explicit identification and measurement can intervention organizations and governments begin to make substantial progress to reduce childhood malnutrition.

6 Conclusion

6.1 Study Summary

This dissertation provides both theoretical and practical insights. Chapter 1 lays the groundwork by introducing the motivation, framework, objectives and key research questions. Chapter 2 introduces anthropometric evaluation of children: the most vital and widely used instrument of public health and clinical medicine. Anthropometry establishes norms, identifies variations, and monitors development. Yet the accurate assessment of physical growth and development of children remains a perpetually beleaguering subject. In chapter 2, I focus on the evolution of anthropometry as a science and its associated measurements, indices, indicators, standards, references, and best practices. I clarify aspects of the assessment of child growth, explore the historical trajectory of the study of anthropometry and its contemporary limitations, and contribute to the debate surrounding references and standards, and the applicability of international anthropometric standards to an individual's health.

Among my findings is a contested record of events, up to and including leading contemporary practices and datasets. I contextualize the legacy of child malnutrition studies in a broad framework, including the linkage between eugenics and contemporary notions of “normal” children. I show the pertinacious competition among individuals and institutions to become the preeminent child growth authority. I propose a distinction between reference growth charts and standards of growth, and I illustrate the unforeseen consequences of universal growth standards that no longer reflect any observable populations.

I recommend that moving forward we should continue to interrogate contemporary manifestations of anthropometric ontologies. This essay illustrates the need for future studies of anthropometry to re-evaluate the orthodoxy. In particular, more studies that grapple with the incipient motivations of anthropometry as a science and its resultant legacy are needed in addition to studies that explore the consequences of universal growth standards. The social determinants of health will be better understood through more prodigious and progressive meta-histories of anthropometry.

Chapter 3 reveals that malnutrition devastates millions of children globally every year, yet the consensus of determining factors remains mixed and obscure. Based on a systematic literature search, I review 184 disaggregate empirical studies of the determinants of childhood malnutrition in Africa published since 1990. This collection spans 30 years, includes 34 countries, and is the largest and most comprehensive review of its kind to date.

I show that the literature concerning disaggregate empirical studies of childhood malnutrition is found wanting for answers to two essential questions: What are the determinants of malnutrition? And how much do the determinants affect malnutrition? I show that the role of spatial heterogeneity, hierarchical institutions, and divergent causal pathways of various non-illness related latent determinants is small but growing. I find an over emphasis of stunting and that few studies consider conflict and environment etiologies despite being the primary factors attributed to malnutrition, hunger, and death in most catastrophic famine events.

Despite the extensive body of literature, my findings highlight a number of opportunities for future research. I find that a lack of comprehensive and high-quality data is a non-binding constraint. The primary need for future research is for more rigorous and practically useful findings of epidemiologically significant determinants. I recommend that more studies are needed with broad temporal, spatial, and hierarchical perspectives, a more exhaustive set of nutrition outcomes, and findings that are quantifiable and epidemiologically significant. There is an untapped wellspring of opportunity for future research within this realm of social determinants of child malnutrition. Not only will a renewed focus on size-matters, epidemiologically significant findings prove academically fruitful, it will also be enlightening practically for practitioners and policy makers.

Chapter 4 returns to the science of anthropometry. I find that many practitioners have adopted a questionable quality control maxim for judging anthropometric surveys. I can find no published study which properly substantiates the maxim; however, the practice is pervasive. The practice is endemic with harmful consequences. I show practitioners who endorse the maxim transpose the conditional and muddle samples with populations and references with standards. Throughout chapter 4, I detail the genesis and propagation of the maxim in the literature, expose its theoretical and logical weaknesses, illustrate its demerits, and offer an alternative approach. Chapter 4 serves to illustrate the consequences first shown in chapter 2 and builds upon the foundation laid down by chapter 3. Specifically, statistically “normal” universal growth charts, the casual interchange of growth references and standards, the hazards of unscrutinized methodology, and the paradoxical nature of translating

between qualitative and quantitative determinants. I show that indeed the literature would benefit from more focus on size-matters findings. More studies that explore the nature of quality control mechanisms and practical *post hoc* measurement methodologies are needed.

Chapter 5 turns its gaze towards the business of modeling the social determinants of health. From earlier chapters, I find that quantifiable impacts of many determinants remain obscure. I combine environmental, health, and demographic data from three rounds of Kenya and Nigeria Demographic Health Surveys. I use four-level random intercept hierarchical generalized logit models to evaluate the responsiveness of malnutrition indicators. I find spatial and hierarchical relationships explain 28-36 percent of malnutrition outcome variation. Furthermore, precipitation, temperature, or vegetation alone can move malnutrition rates by more than 50%. I determine wasting is most impacted by mother's education, family wealth, clinical delivery, and vaccinations; while stunting is most impacted by family wealth, mother's education, clinical delivery, vaccinations, and children asymptomatic of fever, cough, or diarrhea. The present work has many implications for policymakers and researchers. I showcase the scope and scale heterogeneity of climatic determinants and the divergent causal pathways across malnutrition outcomes. This finding should emphasize the need for broad multifactor assessments of health that cover a broad range of outcomes.

Considerable opportunities exist to expand this research to include more countries and more nuanced structural frameworks to better understand the varied and complex social determinants of child malnutrition. Specifically, as I demonstrate in

chapter 3, more attention is needed across a diversity of malnutrition outcomes and with a particular renewed examination within context and countries where malnutrition is most severe. While more and better data is necessary for certain places, a plentiful supply for many of the most severely impacted places already exists. However, many etiologies of conflict, climate, and seasonality confounders remain underexplored. I have shown in chapter 3 that by far stunting is over emphasized and increasingly so throughout the literature, however, my results in chapter 5 illustrate that wasting is by far more responsive to climactic disturbances. Future efforts with better identification strategies and panel data methodologies would go a long way to illuminate these unknowns.

6.2 Final Thoughts

The research presented here represents a brief and momentary endeavor. It is the nascent burgeoning from an erstwhile conversation (Burke, 1973). As anthropometry practices evolve, economic growth persists, public health initiatives are implemented, and each is disrupted by ever worse and uncertain climate impacts, new strategies and approaches to child malnutrition may evolve. This reality emphasizes the importance of continued introspection, debate, and reevaluation of the comprehensive framework approach (Figure 1).

Scholarship is argument, and argument is rhetoric: in the ancient sense of discourse and unforced persuasion (Nelson et al., 1987). The heuristic potential of analogical discourse is considerable (Gusfield, 1976; Hesse, 1966; McCloskey, 1998). All too many studies close with prosaic recitations of more data, better data, and making future impacts with vague policy initiatives. Critique is what

fundamentally advances science (Feyerabend, 1987; Fleck, 1979; Kuhn, 1962; Polanyi, 1958). So, too, do geographer's reinforce the authority of their assertions using their traditional craft-skills as rhetorical devices of persuasion (Golinski, 1990).

The history of scientific thought shows that geography's institutional infrastructure is not fixed and the nature of geography is always negotiated. The geographer and historian David Livingston contends, "The idea that there is some eternal metaphysical core to geography independent of historical circumstances will simply have to go" (1993, p. 28). Geographic knowledge is a product of its time; it reflects its contemporaneous social, economic, and political environment (Harvey, 1984). By understanding how geography's intramural domain and extramural domain interlace, one begins to understand how the methods employed by geographer's are vast and context dependent (Livingston, 1993).

The final outcome of this dissertation is to demonstrate that basic research, close reading, historical appraisal, epistemological introspection, and critical discourse are necessary and sufficient components of science and scientific advancement. The pinnacle of scholarship is to produce work that is simultaneously useful, interesting, unexpected, novel, and above all true. Transcendent scientific endeavors bestow incremental knowledge in each facet. Scholarship that achieves even one of these dimensions is a worthy undertaking. If I have been able to impart even a modicum of progress within this field, I will be content.

7 Appendices

7.1 Appendix A

7.1.1 Final Search Criteria

Finalized search executed on February 23, 2021.

TOPIC:

((stunting) OR (stunted) OR (wasting) OR (wasted) OR (underweight)) AND (child*) AND (determin*) AND ((Africa) OR (African) OR (sahara) OR (saharian) OR (sub-sahara) OR (sub-saharian) OR (Sahel) OR (Algeria) OR (Angola) OR (Benin) OR (Botswana) OR (Burkina Faso) OR (Burundi) OR (Cameroon) OR (Cape Verde) OR (CAR) OR (Central African Republic) OR (Chad) OR (Comoros) OR (DRC) OR (Democratic Republic of the Congo) OR (Republic of the Congo) OR (Congo) OR (Djibouti) OR (Egypt) OR (Equatorial Guinea) OR (Eritrea) OR (Ethiopia) OR (Gabon) OR (Gambia) OR (Ghana) OR (Guinea) OR (Guinea-Bissau) OR (Ivory Coast) OR (Côte d'Ivoire) OR (Kenya) OR (Lesotho) OR (Liberia) OR (Libya) OR (Madagascar) OR (Malawi) OR (Mali) OR (Mauritania) OR (Mauritius) OR (Morocco) OR (Mozambique) OR (Namibia) OR (Niger) OR (Nigeria) OR (Rwanda) OR (Sao Tome and Principe) OR (São Tomé and Príncipe) OR (Senegal) OR (Seychelles) OR (Sierra Leone) OR (Somalia) OR (South Africa) OR (South Sudan) OR (Sudan) OR (Swaziland) OR (Tanzania) OR (Togo) OR (Tunisia) OR (Uganda) OR (Zambia) OR (Zimbabwe))

LANGUAGE:

(English) AND

DOCUMENT TYPES:

(Article) AND

INDEXES:

((SCI-EXPANDED) OR (SSCI) OR (A&HCI) OR (ESCI)) AND

TIMESPAN:

(1990-2020)

Results = 903 Articles

7.1.2 Expanded Search Criteria

TOPIC:

((malnutrition) OR (malnourished) OR (undernutrition) OR (undernourished) OR (stunting) OR (stunted) OR (wasting) OR (wasted) OR (linear growth) OR (faltering) OR (retardation) OR (cachexia) OR (kwashiorkor) OR (underweight) OR (BMI) OR (Body Mass Index) OR (length/height-for-age) OR (length/height for age) OR (height for age) OR (height-for-age) OR (length for age) OR (length-for-age) OR (HAZ) OR (weight for length) OR (weight-for-length) OR (weight for height) OR (weight-for-height) OR (HAZ) OR (weight for age) OR (weight-for-age) OR (WAZ) OR (z score*) OR (z-score*)) AND

TOPIC:

((Africa) OR (African) OR (sahara) OR (saharian) OR (sub-sahara) OR (sub-saharian) OR (Sahel) OR (Algeria) OR (Angola) OR (Benin) OR (Botswana) OR (Burkina Faso) OR (Burundi) OR (Cameroon) OR (Cape Verde) OR (CAR) OR (Central African Republic) OR (Chad) OR (Comoros) OR (DRC) OR (Democratic Republic of the Congo) OR (Republic of the Congo) OR (Congo) OR (Djibouti) OR (Egypt) OR (Equatorial Guinea) OR (Eritrea) OR (Ethiopia) OR (Gabon) OR (Gambia) OR (Ghana) OR (Guinea) OR (Guinea-Bissau) OR (Ivory Coast) OR (Côte d'Ivoire) OR (Kenya) OR (Lesotho) OR (Liberia) OR (Libya) OR (Madagascar) OR (Malawi) OR (Mali) OR (Mauritania) OR (Mauritius) OR (Morocco) OR (Mozambique) OR (Namibia) OR (Niger) OR (Nigeria) OR (Rwanda) OR (Sao Tome and Principe) OR (São Tomé and Príncipe) OR (Senegal) OR (Seychelles) OR (Sierra Leone) OR (Somalia) OR (South Africa) OR (South Sudan) OR (Sudan) OR (Swaziland) OR (Tanzania) OR (Togo) OR (Tunisia) OR (Uganda) OR (Zambia) OR (Zimbabwe))

TOPIC:

((child*) OR (pediatric) OR (infant*) OR (baby) OR (babies) OR (five years) OR (5 years) OR (59 month*) OR (fifty-nine month*) OR (60 month*) OR (sixty month*) OR (2 year*) OR (two year*) OR (1000 days) OR (1,000 days) OR (one thousand days) OR (youth) OR (young)) AND

TOPIC:

((etiolog*) OR (cause*) OR (factor*) OR (determin*) OR (correlat*) OR (disaggregat*) OR (empiric*)) AND

LANGUAGE:

(English) AND

DOCUMENT TYPES:

(Article) AND

INDEXES:

((SCI-EXPANDED) OR (SSCI) OR (A&HCI) OR (ESCI)) AND

TIMESPAN:

(1990-2020)

Results = 13,893 (as of February 23, 2021)

7.2 Appendix B

7.2.1 SD as QC in the Literature

The practice of SD as QC is pervasive, almost to the point of being a norm or a given first principle of the field where citation and evidence are not required. And I believe that the SD as QC maxim is preventing more studies and surveys from being used and published. In Google Scholar, Mei and Grummer-Strawn (2007) are cited over 170 times, not to mention the over 8,950 articles citing WHO (1995) or the 760 citing de Onis and Blössner (1997). Clearly not all are relevant to the SD as QC discussion.

To help illustrate the point I spent an afternoon tracking down articles that explicitly and openly abide by the SD as QC maxim in some form or another. Below are excerpts from a sample of 32 articles citing Mei and Grummer-Strawn (2007) where authors point to the SD as QC maxim. I have put some words in boldface for emphasis.

“Researchers also have analyzed ways in which use of the WHO standards might affect prevalences of wasting, stunting, and underweight worldwide, as well as **the distribution of z scores, a commonly used indicator of data quality** in international surveys” (Grummer-Strawn et al., 2010, p. 13).

“Accepted best practices for field-level quality control were followed. Systematic repeat data entries were done for all anthropometric data. Postanalysis **quality checks compared SDs of anthropometric data by site to WHO standards** and other studies for children <2 y of age” (Remans et al., 2011, p. 1636).

“There were another 5,010 children whose length-for-age z-scores (LAZs) were flagged in the DHS data files either as missing or as biologically implausible according to the WHO flags (Mei & Grummer-Strawn, 2007). **These children were excluded from the analysis.** We also removed 71 children whose mothers had a height of less than 130 cm, as these were considered to be implausible and likely due to measurement or recording errors” (Krasevec et al., 2017, p. 2).

“z score **SDs were within the valid range** accepted by the World Health Organization (WHO)” (Corvalán et al., 2009, p. 548).

“Summary statistics showed that **standard deviations** of the three indices Z score (weight for age, height for age and weight for height) were **between 0.92 and 1.03, indicating high quality data**” (El Mouzan et al., 2008, p. 339).

“The data were subjected to post-hoc methods of quality determination, and, if of suitable quality, included in the adequacy evaluation. ... Accepted practices for field-level quality control were followed. However, systematic repeat measures, repeat sampling and inter-lab sampling were not available for quality control of the MICA data. Therefore alternative, post-hoc methods were used for evaluating the quality of data collected. Some of these methods have been used previously, whereas others were developed for the purpose of this evaluation. ... Comparison of magnitude of SDs of continuous variables to SDs in other, well-controlled studies... This method of **comparing SDs with reference populations has been recommended for anthropometrics**. We assume that common levels of variations will exist for other variables. ... SDs of continuous variables in MICA surveys in baseline (1996 or 1997), follow-up (2000) and endline (2004) compared with examples from the literature, for quality control purposes” (Berti et al., 2010, pp. 613, 617, 618).

“In the analysis, plausibility of anthropometric Z scores were checked using the WHO protocol recommendations (2006), which provide **standard deviation cut points for anthropometric Z-scores as a data quality assessment tool**” (Abate & Belachew, 2017, p. 6).

“Mei and colleagues previously reported a lack of a relationship between SD and mean HAZ across DHS surveys; however, they did not quantitatively assess the change in SD with the age-related decline in mean HAZ, and **they interpreted their findings only as a justification for using SD as an indicator of anthropometric survey quality**” (Roth et al., 2017, p. e1255).

“Mei and Grummer-Strawn [2007] supported the use of **SD as a quality indicator** for anthropometric data” (Afifi et al., 2012, p. 2655).

“In our opinion reports from **surveys with an SD of more than 1.2 are unreliable**. ... An analysis of DHS and MICS shows **elevated SD values with all of the mean SDs outside the acceptable range; none of mean SDs for any of the surveys was less than 1.0Z**. In agreement with the data from West Africa, the 5th and 95th centiles of the SDs of 51 recent DHS surveys were HAZ 1.35–1.95; WAZ 1.17–1.46, and WHZ 1.08–1.50. Mei & Grummer-Strawn conclude that they ‘concur with **the WHO assertion that SD is in a relatively small range**’” (Grellety & Golden, 2016, p. 19).

“Before turning to multivariate regressions, we relate our results to two indicators of measurement error used in previous work. The first step is to compare our December–

January gap with the SD of HAZ. The SD of HAZ could reflect genuine dispersion related to health inequality but is **widely used as an indicator of survey errors** in both height and age (Assaf et al. 2015; Mei and Grummer-Strawn 2007)” (Larsen et al., 2019, pp. 716-717).

“**Standard recommendations state that a standard deviation of greater than 1.3 for HAZ reflects excessive random variation** in either height measurements or age estimates. The standard deviation of HAZ in the three DHS greatly exceeds this threshold for data quality; however, this recommendation is based on the use of the old NCHS:CDC:WHO reference population. There is evidence that standard deviations for HAZ greater than 1.3 are common in DHS in other countries and may be normal when using the WHO Child Growth Standard (Mei & Grummer-Strawn 2007)” (Woodruff et al., 2017, p. 15).

“Many DHS surveys have **standard deviations greatly exceeding the quality criteria** defined by the World Health Organization. ... Ranges are then used to describe the overall quality of the survey and arbitrary cut-offs are used to decide whether the data are acceptable or not” (Tuffrey & Hall, 2016, pp. 4-5, 14).

“We calculated z-score standard deviations (SD) and analyzed SD disaggregated by age (under and over two years of age) **to determine if the quality of measurements** differed by age. ... We can consider z-score standard deviation to illustrate the importance of reaching consensus on interpretation and action. WHO and the US CDC promote the use of normative ranges of SD to determine if survey quality is acceptable, but the ranges are based on surveys that have evidence of poor data quality. The most recent DHS data quality assessment showed that 30 of 52 countries had HAZ SD greater than 1.5, but only one country suppressed data because of poor quality. According to SMART **data quality is not acceptable if HAZ SD is above 1.2**, and a recent modeling study showed that SD of 1.5 can result in substantial overestimation of stunting prevalence. Meanwhile, the published normative range for HAZ SD that some organizations use to deem data quality acceptable is 1.35–1.95” (Conkle et al., 2017, pp. 5, 10).

“Few studies have assessed the distribution of WFH. Two looked at the standard deviations of the WFH distributions. In 1977, **Waterlow et al. showed that the WFH distributions were skewed at the upper centiles**. Their analysis was performed on data from surveillance or surveys involving nutrition and anthropometry in young children up to the age of 10 years. In 2006, Mei et al. analysed data from 51 DHS surveys representing 34 developing Countries. They found a mean WFH and SD WFH (z-scores) of 0.06 and 1.40 respectively. The mean ranged from –0.91 to 0.83 and the SD range [*sic*] from 1.03 to 1.55. **They concluded that their analysis confirms the WHO assertion that the SD remains in a relatively small range (i.e. close to SD from a standard normal distribution)**, no matter the Z-score mean although the observed range of SD for was [*sic*] consistently wider” (Frison et al., 2016, p. 7).

“Summary statistics showed SDs of the 3 indices’ Z score (weight for age, height for age, and weight for height) between 0.92 and 1.03, **indicating high-quality data**” (El Mouzan et al., 2009, p. 68).

“Previous research has demonstrated that Z-scores within a population are normally distributed with a SD of approximately 1.0; the shape of the distribution does not vary based on the nutritional status of the population, as measured by the mean Z-score. Based on the finding that SD remains in a relatively narrow range for each indicator regardless of mean Z-score, **WHO guidance recommends that the SD of Z-scores can be used as a data quality indicator** as well as a measure of variability. The introduction of random non-directional errors, such as those introduced when age is estimated rather than calculated or when teams are imprecise in measuring height or weight, can result in wider SD relative to the acceptable ranges outlined by WHO. ... We therefore included **SD of the Z-scores to assess the degree to which data quality** in addition to variability impact DEFF in anthropometric surveys. ... The SD of WHZ and WAZ were approximately 1.00, as expected in high-quality anthropometry surveys (WHZ median = 1.03, WAZ median = 1.04)” (Hulland et al., 2016, pp. 2-3, 10).

“Anthropometry **data quality indicators were extremely high (median SDs for weight-for-length, length-for-age and weight-for-age z-scores 1.01, 0.98, and 1.03, respectively)**, likely due to extensive training, standardization, and monitoring efforts. ... Anthropometry data quality indicators were monitored throughout the study. The medians of monthly standard deviations for weight-for-length, length-for-age, and weight-for-age z-scores were 1.01, 0.98, and 1.03, respectively; **close to the expected value of 1.0 for a reference distribution**. Standard deviations for z-scores varied month-to-month, but never reached the WHO thresholds for measurement error or incorrect age reporting” (Aceituno et al., 2017, pp. 2, 8).

“The standard deviations reported in this study are much lower than the **suggested standard deviations** reported by Mei and Grummer-Strawn estimations in a cross-country analysis” (Sharma et al., 2020, p. 17).

“We also examined the quality of the 2009 data by **assessing the SD as a quality indicator** for anthropometric data (Mei and Grummer-Strawn 2007) and examining whether or not age heaping was evident. These assessments did not reveal any concerns” (Boylan et al., 2017, p. 2261).

“Based on the WHO Technical Report, the **SD for Weight-for-Height (WFH) should be between 0.8 and 1.2 Z-score units in all well-conducted surveys**. This has been confirmed empirically with well conducted surveys in both the developed world where large national surveys of heterogeneous populations have been conducted, for example the National Health and Nutrition Examination Survey (NHANES) from USA’s National Centre for Health Statistics (NCHS) and the developing world. ... The SD of organisation “t” differs significantly from the others (Student’s t test < 0.0001), with 69% (53/77) of their surveys for WHZ having **an SD**

of more than 1.2 Z. ... For most anthropometric measurements the SD from single surveys should lie between 0.8 and 1.2, with about 80% between 0.9 and 1.1Z. For these reasons the **SD has been used as a useful measurement of data quality**" (Grellety & Golden, 2018, pp. 2, 3, 10).

"The median SD and range for HAZ were greater overall and across all surveys than for WHZ. The absolute difference in HAZ by MOB of age reporting should be close to 0 if there is no systematic error in age reporting, but was 0.25 (in z score units) overall and up to 0.90 in Timor-Leste in 2009. ... HAZ SD and WHZ SD had the highest factor loadings in the data quality indices indicating that **SD is an important measure of anthropometric data quality**" (Perumal et al., 2020, pp. 809S, 812S).

"Absent measurement error, distributions are expected to be approximately normal with a SD close to 1. ... To exclude surveys with exceptionally poor anthropometry data quality or where data manipulation might be suspected, **we excluded from analysis surveys where the SD for WHZ, WAZ, HAZ, or BMIZ was outside of the following empirically defined cutoffs:** greater than 1.8 or lower than 0.8; or the SD for MUACZ greater than 1.8 and less than 0.7" (Bilukha et al., 2020, pp. 2, 3).

"Anthropometric data collected during the 2008 to 2009 and 2014 Kenya surveys were reanalyzed to assess standard parameters of quality: standard deviation, skewness, and kurtosis of z-score values for 3 anthropometric indicators (weight for height, height for age, and weight for age)... The primary objective of the comparative analysis was to observe the quality of anthropometric variables. The **first metric of quality, standard deviation**, is presented in Table 3. ... One key measure is SD of the continuous z-score distributions. As noted, previous research suggests that for a given population, Z-scores are normally distributed with an SD of approximately 1.0" (Leidman et al., 2018, pp. 406, 412, 414).

"Careful interpretation is required, as the **standard deviations** for our anthropometric measurements are outside the World Health Organization range **for data quality assessment purposes**" (Bennett et al., 2020, p. 2038).

"Note that the standard deviations (SD) of WHZ and MUACZ in all rounds are near or even below 1.0, which gives us **confidence in the quality of the anthropometric data** (Grellety and Golden 2016b; Mei and Grummer-Strawn 2007). The average SD—across all four survey rounds—is 1.03 for WHZ and 0.95 for MUACZ" (Ecker et al., 2019, p. 10).

"Seventeen surveys had large standard deviations (SD) for HAZ, which could result in attenuated regression coefficients when HAZ was used as an explanatory variable in regression analyses. To avoid attenuation, HAZ values for each child were **adjusted to obtain a standard deviation for HAZ of 1.2** for each of these surveys by subtracting the survey mean for HAZ, dividing by the survey SD for HAZ, multiplying by 1.2, and then adding back the survey mean for HAZ" (Frongillo et al., 2017, p. 3038).

“The World Health Organization (WHO) has recommended the use of Z-score of these indicators to classify nutritional status, given the constancy of their values, independent of nutritional status, and can even be used as **indicators of the quality of anthropometric data**” (Martins et al., 2010, p. 1106).

“Z-score plausibility was determined using WHO cutoffs. We used the following WHO-defined **standard deviation (SD) ranges to assess the quality** of data (HAZ 1.1–1.3, WAZ 1.0–1.2, and WHZ 0.85–1.1)” (Gupta et al., 2020, pp. 2-3).

“...as per WHO standards. Some individuals may have met >1 **exclusion criterion**” (Varghese & Stein, 2019, p. 1208).

“Protocol used for obtaining data was an adaptation of that published by Lapham et al. and Mei et al.” (Samiak & Emeto, 2017, p. 2).

“Studies investigating the quality of the DHS data report the quality to be good (Mei Z and Grummer-Strawn LM., 2007, Mishra et al., 2006)” (Reda & Lindstrom, 2014, p. 1160).

7.2.2 Z-score SD Proof

The aim here is to move away from the discussion of z-scores and standard deviations of z-scores to simply anthropometric index measurements and standard deviations of anthropometric index measurements. To make this simplification I will show that a z-score standard deviation is equivalent to the ratio of standard deviations of an anthropometric index to that of the reference population. The standard deviation of a given survey’s anthropometric index is calculated as:

$$s_x = \sqrt{\frac{1}{N-1} \sum_{i=1}^N (x_i - \bar{x})^2}$$

where:

- s_x : anthropometric index sample standard deviation
- N : is the number of children in the sample

- x_i : is a child's anthropometric index value (e.g., weight-for-height)
- \bar{x} : is the anthropometric index sample average given by:

$$\bar{x} = \frac{1}{N} \sum_{i=1}^N x_i$$

A z-score tells you how many standard deviations away an individual data value falls from the mean. It is calculated as:

$$Z_i = \frac{(x_i - \mu)}{\sigma}$$

where:

- Z_i : is a child's z-score
- x_i : is a child's anthropometric index value (e.g., weight-for-height)
- μ : is the reference mean
- σ : is the reference standard deviation

A given survey's z-score standard deviation is calculated as:

$$s_Z = \sqrt{\frac{1}{N-1} \sum_{i=1}^N (Z_i - \bar{Z})^2}$$

where:

- s_Z : z-score sample standard deviation
- N : is the number of children in the sample
- Z_i : is a child's z-score
- \bar{Z} : sample average z-score given by

$$\bar{Z} = \frac{1}{N} \sum_{i=1}^N Z_i$$

Thus, we are left with the question:

Is the statement, *if an anthropometric survey has a z-score standard deviation greater than 1.3 it fails the test*, equivalent the statement, *if the sample standard deviation of an anthropometric index is 1.3 times that of the standard deviation of the reference population it fails the test?*

Or in other words, is the ratio of the sample standard deviation of (weight-for-height) to the reference population standard deviation of (weight-for-height) equivalent to the standard deviation of (weight-for-height) z-scores.

Claim:

$$\sqrt{\frac{1}{N-1} \sum_{i=1}^N (Z_i - \bar{Z})^2} = \frac{\sqrt{\frac{1}{N-1} \sum_{i=1}^N (x_i - \bar{x})^2}}{\sigma}$$

Squaring both sides and reducing gives:

$$\sum_{i=1}^N (Z_i - \bar{Z})^2 = \frac{1}{\sigma^2} \sum_{i=1}^N (x_i - \bar{x})^2$$

Note x_i is a random variable and μ and σ are constants such that $Z_i = \frac{(x_i - \mu)}{\sigma} = \frac{-\mu}{\sigma} +$

$\frac{1}{\sigma} x_i$ is a linear transformation of the form $Z_i = a + bx_i$.

If $Z_i = a + bx_i$ then,

$$E[Z_i] = E[a + bx_i] = a + bE[x_i] = a + b\bar{x}$$

and

$$Var[Z_i] = Var[a + bx_i] = b^2 \sigma_x^2$$

where

$$\frac{1}{N} \sum_{i=1}^N (Z_i - \bar{Z})^2 = \sigma_Z^2 = \text{Var}[Z_i]$$

and

$$\sigma_x^2 = \frac{1}{N} \sum_{i=1}^N (x_i - \bar{x})^2$$

giving

$$\frac{1}{N} \sum_{i=1}^N (Z_i - \bar{Z})^2 = \sigma_Z^2 = b^2 \sigma_x^2 = b^2 \frac{1}{N} \sum_{i=1}^N (x_i - \bar{x})^2$$

Note for our purposes $b = \frac{1}{\sigma}$ such that $b^2 = \frac{1}{\sigma^2}$ giving

$$\frac{1}{N} \sum_{i=1}^N (Z_i - \bar{Z})^2 = \frac{1}{\sigma^2} \frac{1}{N} \sum_{i=1}^N (x_i - \bar{x})^2$$

which reduces to

$$\sum_{i=1}^N (Z_i - \bar{Z})^2 = \frac{1}{\sigma^2} \sum_{i=1}^N (x_i - \bar{x})^2$$

QED

7.2.3 Quantifying the SD Rule

Although the maxim is widely practiced, it is not always consistent. WHO suggests the z-score “distribution should be relatively constant and close to the expected value of 1.0 for the reference distribution” (1995, p. 218). de Onis and Blössner, citing WHO (1995), claim good quality SD ranges of HAZ (1.10 to 1.30), WAZ (1.00 to 1.20) and WHZ (0.85 to 1.10) and state these values are “the expected ranges of

standard deviations of the z-score distributions for the three anthropometric indicators” (1997, p. 51). de Onis and Blössner also state that “[a]ny standard deviation of the z-scores above 1.3 suggests inaccurate data” (1997, p. 51).

Golden and Grellety, suggest “The spread of the standard deviations ... was small; ranging from 0.8 to 1.2 in 95% of the surveys” (2002, p. 5). Grellety and Golden, citing WHO (1995) and Golden and Grellety (2002), state “the SD for Weight-for-Height (WFH) should be between 0.8 and 1.2 Z-score units in all well-conducted surveys, with about 80% between 0.9 and 1.1Z” (2018, p. 2).

Mei and Grummer-Stawn, citing WHO (1995), present the same example z-score table of HAZ (1.10 to 1.30), WAZ (1.00 to 1.20) and WHZ (0.85 to 1.10) and claim these values are a “recommendation from a WHO expert panel” as the “ranges for data quality assessment” (2007, p. 445). Mei and Grummer-Stawn (2007) also suggest the ranges for data quality assessment should be wider, given by HAZ (1.35 to 1.95), WAZ (1.17 to 1.46) and WHZ (1.08 to 1.50).

We are told by USAID “that high quality anthropometric data should be normally distributed with a standard deviation of approximately 1” (2016, p. 15). But later USAID informs us that “very large standard deviations, for example greater than 2, might be a sign of poor quality” (2016, p. 15).

Bilukha et al., citing WHO (1995) and WHO and UNICEF (2019), give the recommendation that “Absent measurement error, distributions are expected to be approximately normal with a SD close to 1” (2020, p. 2). However, Bilukha et al. choose the exclusion criteria of “greater than 1.8 or lower than 0.8” (2020, p. 3).

7.2.4 The Multicentre Growth Reference Study

The Multicentre Growth Reference Study (July 1997–December 2003) consists of both cross-sectional and longitudinal surveys from six cities: Davis, California, USA; Muscat, Oman; Oslo, Norway; Pelotas, Brazil; in select affluent neighborhoods in Accra, Ghana; and South Delhi, India (WHO, 2006b). The distributions of children across the different survey countries for the longitudinal component are: 119 USA; 149 Oman; 148 Norway; 66 Brazil; 227 Ghana; and 173 India. The distributions of children across the different survey countries for the cross-sectional component are: 476 USA; 1,438 Oman; 1,385 Norway; 480 Brazil; 1,403 Ghana; and 1,487 India.

Prior to constructing the standards, if a child was 3 SDs above the sample median or 3 standard deviations below the sample median they were excluded. For the cross-sectional sample the truncation procedure was even stricter. If a child was 2 SDs above the sample median or 2 SDs below the sample median they were excluded. Children were selected for inclusion based on: no known health or environmental constraints to growth, mothers willing to follow feeding recommendations, no maternal smoking before and after delivery, single term birth, and absence of significant morbidity. Of the 13,741 children screened for the longitudinal survey, less than 7% or 882 children (428 boys and 454 girls) were eligible and included in the final study. In addition, of the 21,520 children screened for the cross-sectional survey, less than 31% or 6,669 children (3,450 boys and 3,219 girls) were eligible, and included in the final study. In other words, 93% to 69% of the populous did not fit the standard.

After selective sampling and exclusion, the sample was exceedingly skewed to the right (WHO, 2006b). To rectify the non-normality, the data were cleaved at the median, and then reflected to create two symmetrical distributions. Each mirrored distribution was used to derive standard deviation cut-off values (i.e., what is the severe wasting cutoff value where a WHZ score is less than 3 SDs from the median) for the respective upper and lower portions of the data.

7.2.5 Transposed Conditional and Affirmed Consequent

The *fallacy of the transposed conditional*, also known as *confusion of the inverse* or the statistical equivalent to the *fallacy of affirming the consequent*, is the jumbling of the probability of a set of data given a hypothesis, and the probability of a hypothesis given a set of data.

In statistical terms, the *fallacy of the transposed conditional* is corroborated through Thomas Bayes' (1763) theorem, given by:

$$\Pr(A|B) = \frac{\Pr(B|A)\Pr(A)}{\Pr(B)}$$

where A and B are two different outcomes or events (i.e., a hypothesis and a data set) and $\Pr(B) \neq 0$. Therefore, we can see $\Pr(A|B) = \Pr(B|A)$ holds true if and only if $\Pr(A) = \Pr(B)$ at the same time.

It is a fallacy if one claims to test the likelihood of a null hypothesis assuming the data are true, if what is actually tested is the likelihood of the data assuming the null hypothesis is true. It is incorrect to assume $\Pr(\text{Data}|H_0) = \Pr(H_0|\text{Data})$.

In terms of rhetoric and logic, the *fallacy of affirming the consequent* is stated:

$$\frac{P \rightarrow Q, Q}{\therefore P}$$

where one takes the true statement $P \rightarrow Q$ and incorrectly concludes the converse $Q \rightarrow P$ to be true. In plain terms, the fallacy is demonstrated with the simple and absurd statement: *All dogs are animals; therefore, all animals are dogs.*

7.3 Appendix C

7.3.1 Conceptual Frameworks in Context

The 1990 UNICEF multisectoral framework encompasses food, health, and caring practices to help identify the most appropriate mixture of actions (UNICEF, 1998). The original presentation warns that the UNICEF conceptual framework is not a predictive model, but instead a deliberately flexible model adaptable to different prescriptive and causal contexts. The emphasis of the model is on accommodating many possible determinants of malnutrition and prioritizing the most important within a specific contextual application while being easy to communicate across different users (UNICEF, 1990).

Since its inception the UNICEF conceptual framework has been the standard for modeling the broad causes of child malnutrition. And it has been adapted into many new interpretations (e.g., Blessing J. Akombi, Kingsley E. Agho, John J. Hall, et al., 2017; Blessing J. Akombi, Kingsley E. Agho, Dafna Merom, et al., 2017; Black et al., 2008; Boah et al., 2019; Brown, 2008; Darteh et al., 2014; de Groot et al., 2017; Engebretsen et al., 2008; Engle et al., 1999; Fernandes et al., 2017; Fernandez et al., 2002; Habaasa, 2015; Kavle et al., 2015; Lesiapeto et al., 2010; Müller & Krawinkel, 2005; Ricci et al., 2019; Smith & Haddad, 2015; Stewart et al., 2013; UNICEF, 1998, 2013; Wamani et al., 2006; WHO, 2014; Willey et al., 2009).

But by no means is the UNICEF framework the only contemporary framework of malnutrition. Other conceptualizations focus on other factors such as food security (e.g., Akinyele, 2009; Brown et al., 2008; Grace et al., 2012; Stamoulis & Zezza, 2003; Von Braun et al., 1999), risk factors (e.g., Bhutta et al., 2008; Black

et al., 2013; Chopra, 2003; Dearden et al., 2017; Griffiths et al., 2004; Jolliffe, 1962; Mehta et al., 2013; Sastry, 1997; Victora et al., 1997; Walker et al., 2011), national economic growth (e.g., Rashad & Sharaf, 2018; Subramanyam et al., 2011), spatial composition (e.g., Grace, 2017; Khatab, 2010; Smith et al., 2000), and utility maximization (e.g., Chirwa & Ngalawa, 2008; Ssewanyana & Kasirye, 2012) all disseminating from a wider historical epistemology. Simple or incorrect perceptions, however, are often the bases for policy research resulting in mistaken guidance and action (Jonsson, 1993).

More generally, Turner II et al. (2003) in their synthesis and revision of vulnerability analysis build out a systematic conceptual framework structure. The revisionist structural framework models system vulnerabilities, hazards, risks, perturbations, stressors, entitlements, endowments, sensitivities, feedbacks, resilience, and multiequilibria within institutions, heterogeneous subsystems, and social units across various spatiotemporal, nested, and functional scales.

7.3.2 Study Design and Sample Methodology

The Demographic and Health Surveys (DHS) remain the most ubiquitous resource of its kind, with more than 350 surveys in over 90 countries across 30 years. Published, peer-reviewed articles analyzing the Demographic and Health Surveys data have increased precipitously over the last quarter century, contributing to substantial insights into public health around the world (Fabic et al., 2012). The Demographic and Health Surveys comprise seven overlapping phases: DHS-I, 1984 to 1990; DHS-II, 1988 to 1993; DHS-III, 1992 to 1998; DHS-IV, 1997 to 2003; DHS-V, 2003 to

2008; DHS-VI, 2008 to 2013; and DHS-VII, 2013 to 2018. The Surveys have a coarse temporal granularity despite the protracted record.

The Demographic and Health Surveys Program uses calibrated survey instruments, and quality assurance personnel assess collection procedures and administer technician training. The technicians recruit skilled field staff with experience as enumerators. They spend weeks training staff through a detailed, question-by-question explanation of the Questionnaires, and demonstration with role-play, group discussion, and practice interviews. They also provide anthropometry training to all staff to instruct, demonstrate, and practice measuring children (Macro, 2009).

Anthropometric measurements include *weight* (recorded in tenths of a kilogram) and *height* (recorded in tenths of a centimeter). Field staff use specially manufactured measuring boards for survey settings and lightweight digital scales, designed and manufactured under the authority of the United Nations Children's Fund. If a child is younger than two years old, staff measure their height with recumbent length instead of standing height.

Following the World Fertility Survey and the Contraceptive Prevalence Survey projects designed to study reproductive health and household characteristics in developing countries, the United States Agency for International Development (USAID) established in 1984 the Demographic and Health Surveys Program (Rutstein & Rojas, 2006). The Program was first put into effect by Westinghouse Health Systems, which later became part of Macro Systems, ORC Macro, Macro International, and is now executed by the management consulting firm ICF

International and its partner organizations Path, Avenir Health, Johns Hopkins Center for Communication Programs, Vysnova, Blue Raster, Kimetrica, and Encompass (Croft et al., 2018).

The sampling procedure of the Demographic and Health Surveys Program employs a *multistage probability sample design*, drawn from the *sampling frame* of the most recent census. That is to say, the population is partitioned into strata, within which a sample is defined and selected independently. The *sample design* describes the non-zero and predefined probability of every person in the population to be selected for the study, where the *sampling frame* defines subpopulation clusters described by a national census. Regions, zones or provinces stratify national populations, and states or counties stratify regions. The final stratum contains a subpopulation from which to randomly sample clusters. The extent of clusters vary; they can be a city block or apartment building in urban areas whilst being a village or group of villages in rural areas. Generally, geographic regions and urban or rural areas within each region partition the stratified samples of the Demographic and Health Surveys (Burgert et al., 2013).

For example, the *sampling frame* for Nigeria partitions the country into 6 geographical regions, 36 states and the Federal Capital Territory, 774 local government areas, 8812 wards, and 665,000 census enumeration areas, each containing 48 households on average. The *sample design* for Nigeria DHS-VI selected 893 wards with a selection probability proportional to its population and stratified across urban and rural local government areas in each state. The *sample design* then selects 904 census enumeration areas from within the 893 wards and if a

selected enumeration area contains less than 80 households, a neighboring enumeration area is added to form the primary sampling units or clusters. Finally, the *sample design* selects a fixed number of 45 households from each cluster in order to determine who to interview.

The probability of selecting a household is the probability of selecting the cluster multiplied by the probability of selecting the household within the cluster. The overall probability of selecting a household will differ from cluster to cluster. Households per cluster vary across time and country. Kenya DHS clusters all have 25 households whereas Nigeria DHS-VI clusters have 45 households; Nigeria DHS-V clusters have 41 households; and Nigeria DHS-IV clusters have 22 households on average. Nor do the number of clusters remain constant. For example, Kenya DHS-V and DHS-IV have 400 clusters whereas Kenya DHS-VI has 1,612 clusters and Nigeria DHS-IV has 365 clusters; Nigeria DHS-V has 888 clusters; and Nigeria DHS-VI has 904 clusters. The stratified samples produce homogeneity within groups and heterogeneity between groups. The objective of the procedure is to reduce sampling errors and to increase precision and representation (Kenya et al., 2004; KNBS & International, 2015; KNBS & Macro, 2010; Nigeria & International, 2014; Nigeria & Macro, 2009; Nigeria & Macro, 2004).

The Demographic and Health Surveys collect a plethora of population, health, and nutrition statistics from a representative sample of the population. Participating countries are primarily those that receive assistance from the United States Agency for International Development, and surveys are administered in collaboration with country specific partners such as the National Bureau of Statistics, the Ministry of

Health, the National Population Commission, and Medical Research Institutes. Using a standardized questionnaire model, the Demographic and Health Surveys Program aims to collect data that are comparable across countries. Participating countries typically adopt the standardized questionnaires in their entirety. However, the questionnaire model has been modified across each of the seven phases of the Program making it difficult to measure changes through time.

The Demographic and Health Surveys collect data with four main questionnaires. The Household Questionnaire characterizes the household in terms of physical amenities and a roster of the members of the household. The Biomarker Questionnaire characterizes the anthropometric measurements and biochemical indicators of eligible members of the household. Eligible household members are typically children under age 5 and women and men ages 15 to 49. Specific information regarding eligible household members is collected in the Woman's Questionnaire and Man's Questionnaire respectively. Because of specific family planning, reproductive health, and child health subject matter, the Demographic and Health Surveys focus on women of reproductive age.

In addition to characteristics about the woman, the Woman's Questionnaire contains a birth history roster of detailed health and nutrition statistics for select eligible children. The birth history forms the basis for the Kids Recode file, a standardized module containing information related to the child's pregnancy and postnatal care and immunization, health and nutrition data (Croft et al., 2018). The recode file is a standardized file that facilitates cross-country analysis.

The Demographic and Health Surveys Program uses calibrated survey instruments, and quality assurance personnel assess collection procedures and administer technician training. The technicians recruit skilled field staff with experience as enumerators. They spend weeks training staff through a detailed, question-by-question explanation of the Questionnaires, and demonstration with role-play, group discussion, and practice interviews. They also provide anthropometry training to all staff to instruct, demonstrate, and practice measuring children (Macro, 2009).

7.3.3 Variable Composition

7.3.3.1 *Child-level*

The *sex* variable is an unadulterated binary indicator equal to one if the child is female and zero if male. The *delivery* variable is a collapsed binary indicator equal to one if delivery occurred in a hospital facility or health clinic and zero otherwise. The *birth* variable is a collapsed binary indicator equal to one if the delivery was a singleton birth and zero if delivery involved multiple births (i.e., twins). The *weaned* variable is a composite categorical indicator equal to one if the child is weaned by 1 year, two if the child is breastfed up to 1 year, three if the child is weaned before 1 year, and zero if the child is breastfed beyond 1 year. The *vaccines - minimum* variable is a composite binary indicator equal to one if the child received at least 1 of 9 vaccinations (Polio 0, 1, 2, 3; DPT 1, 2, 3; BCG, and Measles) and zero otherwise. The *vaccines - maximum* variable is a composite binary indicator equal to one if the child received all 9 vaccinations and zero otherwise. The *diet* variable is a composite

binary indicator equal to one if the child received a diverse variety of 4 or more food groups (of 7 possible including: grains, legumes, dairy, meat or fish, eggs, fruits and vegetables high in vitamin A, and other fruits and vegetables) in the past 24 hours or 3 or more food groups plus breast milk and zero otherwise. The *sick* variable is a composite binary indicator equal to one if the child is asymptomatic (i.e., did not present with diarrhea, a fever, or a cough in the past 2 weeks) and zero otherwise. The *child's age* variable is an unadulterated continuous indicator of the child's age in months, from date of birth to date of interview.

7.3.3.2 Household-level

The *latrine* variable is a composite binary indicator equal to one if the facility is “improved,” meaning it is not shared and the type of toilet facility for the household is a flush toilet (either to a sewer system, septic tank, pit, or anywhere else); ventilated improved pit latrine or pit latrine with slab; or a composting toilet and zero otherwise if the facility is shared or an open pit; no facility, brush or field; bucket toilet; hanging toilet; or anywhere else. The *water* variable is a collapsed binary indicator equal to one if it is “improved,” meaning the major source of drinking water for the household is piped water into the dwelling, yard, or plot; a public tap, standpipe, or borehole; a protected well or protected spring water; rainwater; or bottled water and zero otherwise from sources including unprotected wells or springs, water delivered by tanker trucks, or surface water. The *mother's education* variable is an unadulterated standardized categorical indicator of highest education level attended equal to zero if no education, one if primary, two if secondary, and three if higher. The *wealth index* variable is an unadulterated composite categorical indicator

of a household's cumulative standard of living, calculated using ownership of assets (e.g., televisions and bicycles); housing construction materials; types of water and latrine facilities, and generated by placing all interviewed households along a continuous scale of relative wealth and then separating them into 5 wealth quintiles: poorest, poorer, middle, richer, and richest. The *mother's age* variable is an unadulterated continuous indicator of the mother's current age in completed years of decades, from date of birth to date of interview. The *birth tally* variable is an unadulterated continuous indicator of the total number of births of the mother.

7.3.3.3 Cluster-level

The *residence* variable is an unadulterated binary indicator equal to one if the *de facto* place of residence is rural (based on whether the cluster is defined as rural in the sample design—a country-specific designation) and zero if defined as urban. The *precipitation* variable is a composite continuous measure of average total monthly rainfall in decimeters during the preceding growing season derived from the Climate Hazards Group InfraRed Precipitation with Station (CHIRPS) dataset replete with a 0.05° spatial resolution (Funk et al., 2015). The *temperature* variable is a composite continuous measure of average maximum monthly temperature in Celsius during the preceding growing season derived from the Climate Hazards Group InfraRed Temperature with Station (CHIRTS) dataset replete with a 0.05° spatial resolution (Funk et al., 2015). The *precipitation anomaly* variable is a composite continuous measure of average monthly rainfall anomaly from the previous five-year average in decimeters during the preceding growing season. The *temperature anomaly* variable is a composite continuous measure of average maximum monthly temperature

anomaly from the previous five-year average in Celsius during the preceding growing season. The *greenness index* variable is a composite continuous unit-less index measure between zero and one of the Normalized Difference Vegetation Index (NDVI) for the three greenest months during the preceding growing season replete with a 0.05° spatial resolution (Vermote et al., 2014).

7.3.3.4 *State-level and Other Controls*

State-level indicators are First-level Administrative Divisions, and include 47 counties for Kenya and 36 states plus one federal capital territory for Nigeria. The *interview month* variable is an unadulterated categorical control indicator of the month in which the survey took place. The *survey phase* variable is an unadulterated categorical control indicator of the phase in which the survey took place (DHS-IV from 1997 to 2003; DHS-V from 2003 to 2008; and DHS-VI from 2008 to 2013) (DHS, 2008, 2012, 2013).

7.3.4 Spatial Dispersions and Distributions

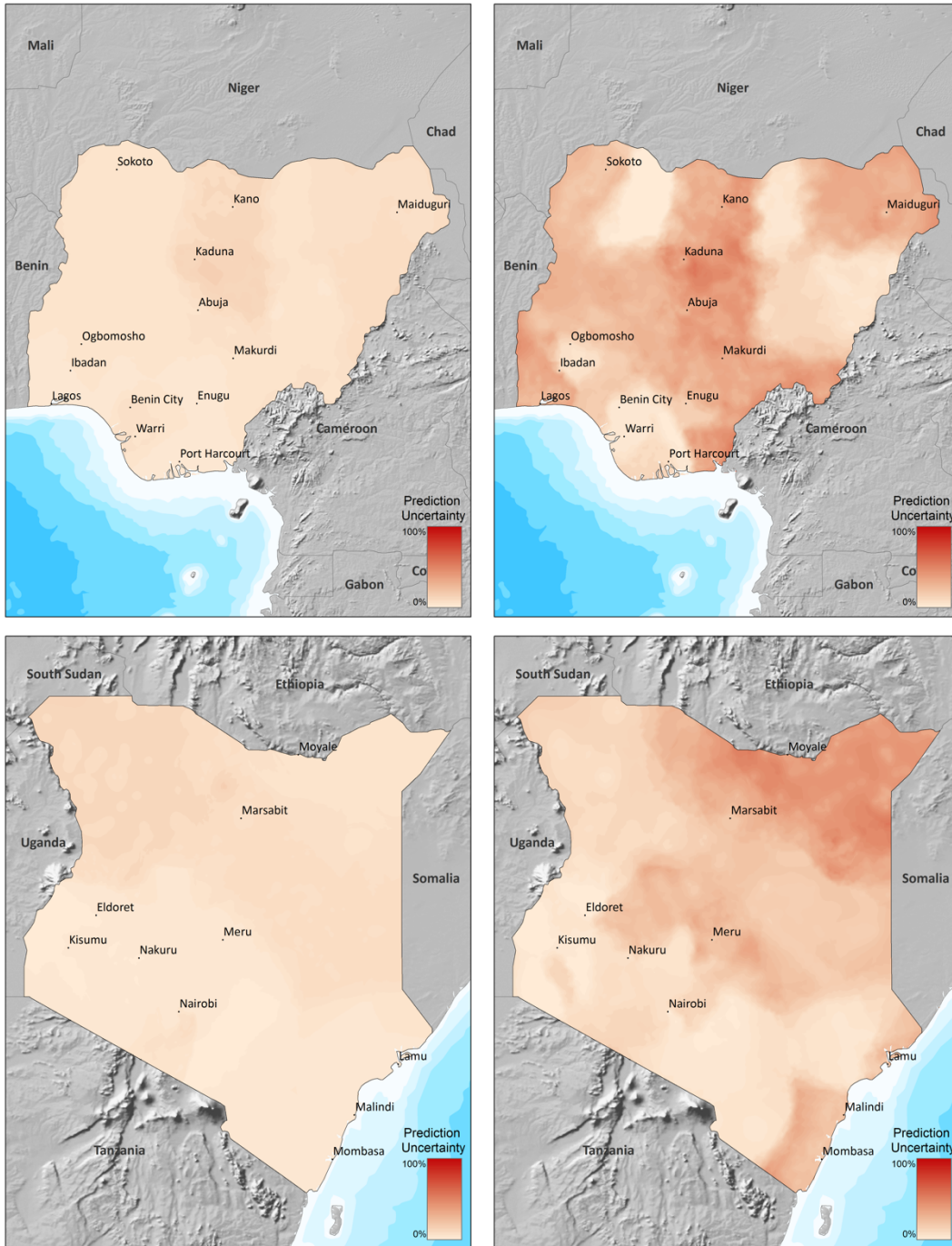


Figure 11: Empirical Bayesian kriging model uncertainty estimates across Kenya and Nigeria based on projections in Figure 6.

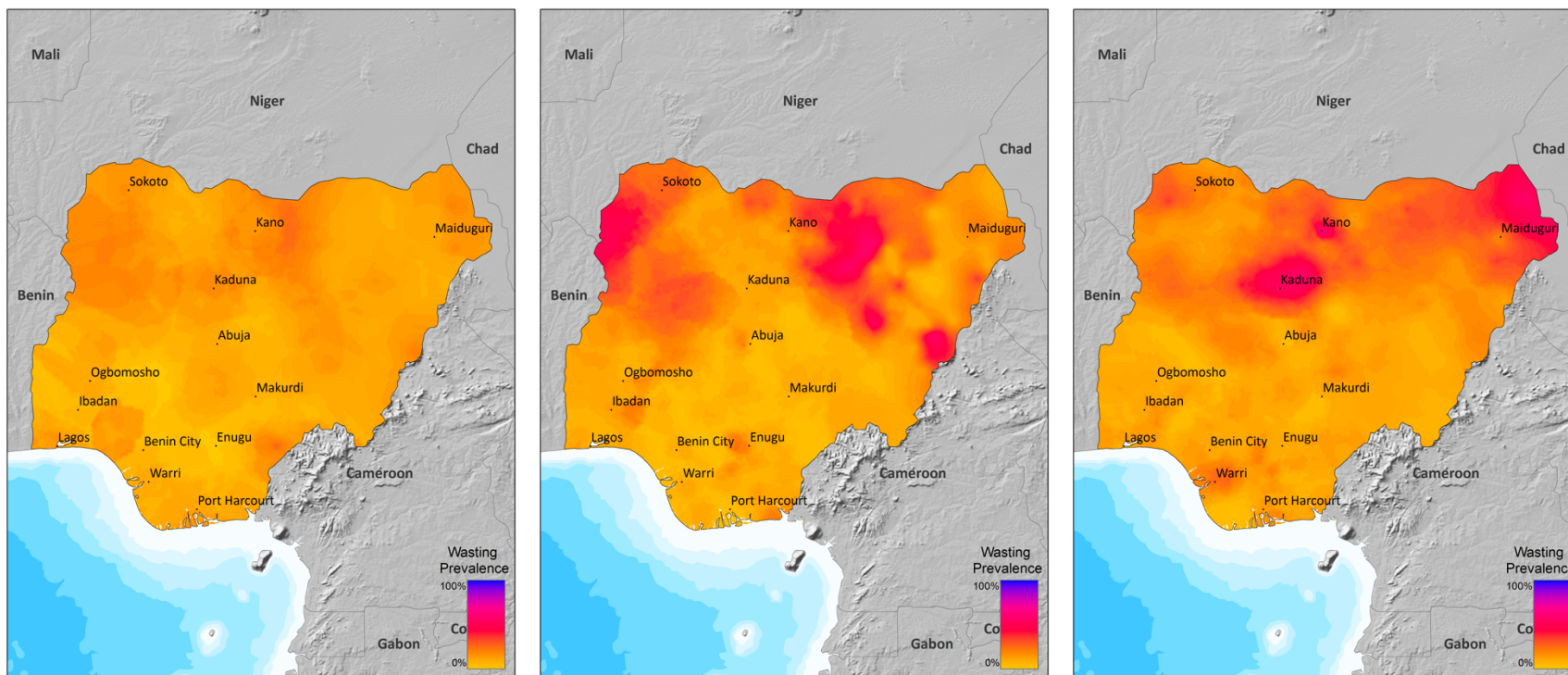


Figure 12: Empirical Bayesian kriging of sample wasting prevalence across Nigeria in 2003, 2008 and 2013 DHS surveys.

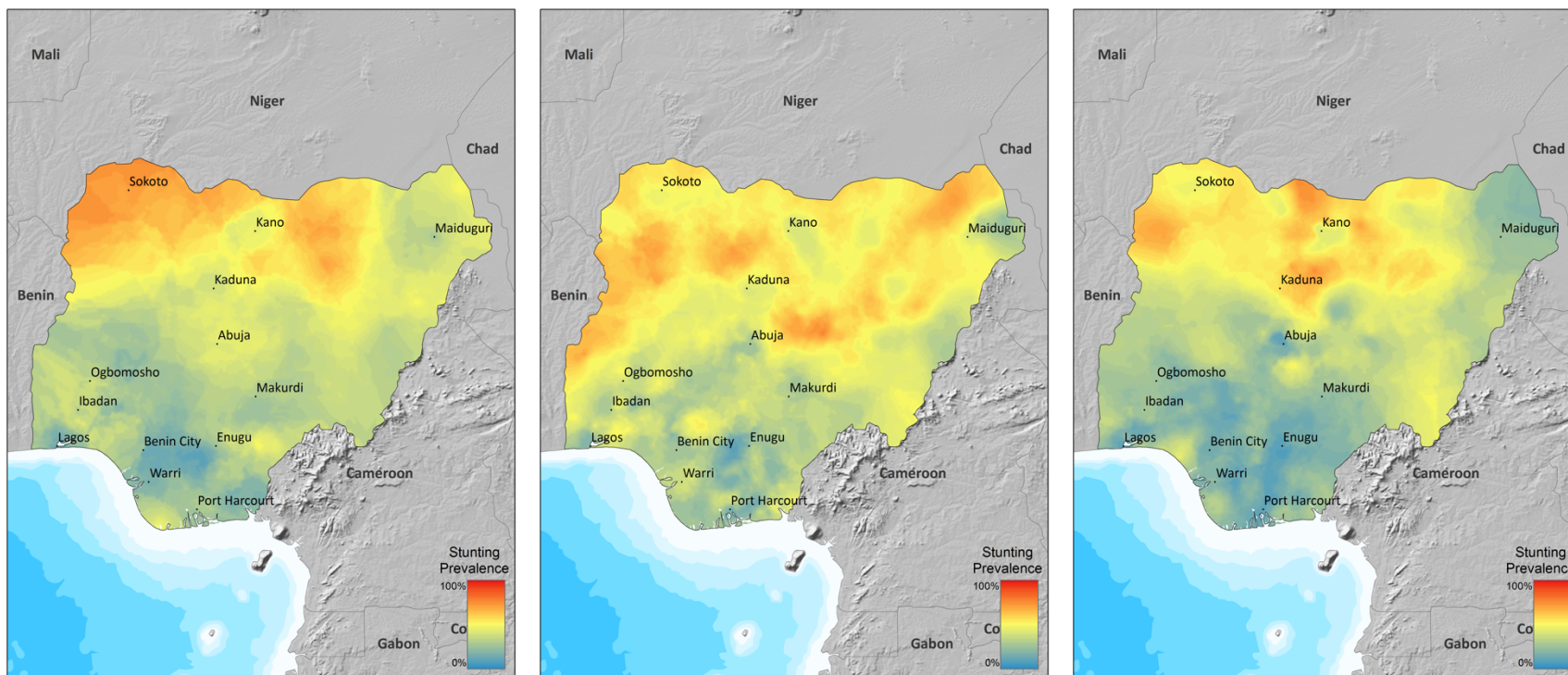


Figure 13: Empirical Bayesian kriging of sample stunting prevalence across Nigeria in 2003, 2008 and 2013 DHS surveys.

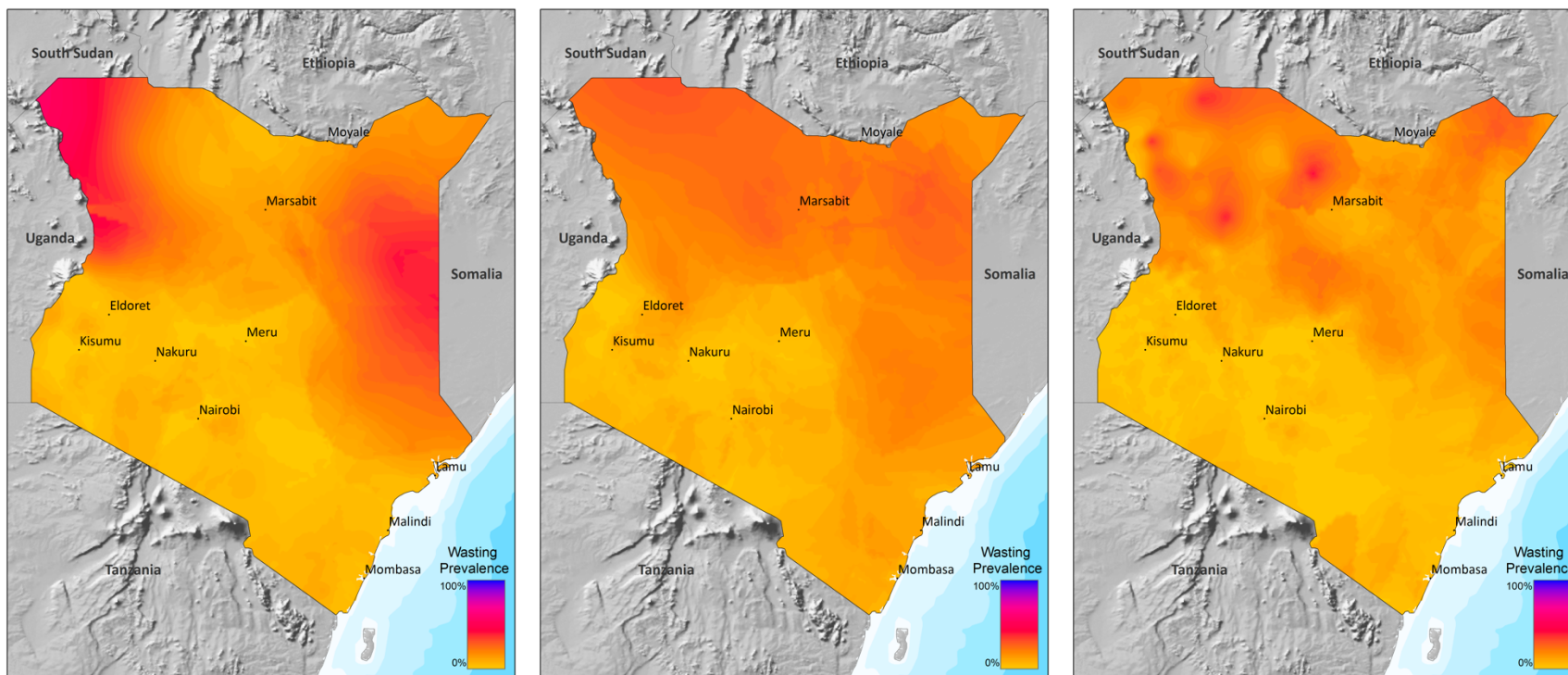


Figure 14: Empirical Bayesian kriging of sample wasting prevalence across Kenya in 2003, 2008 and 2014 DHS surveys.

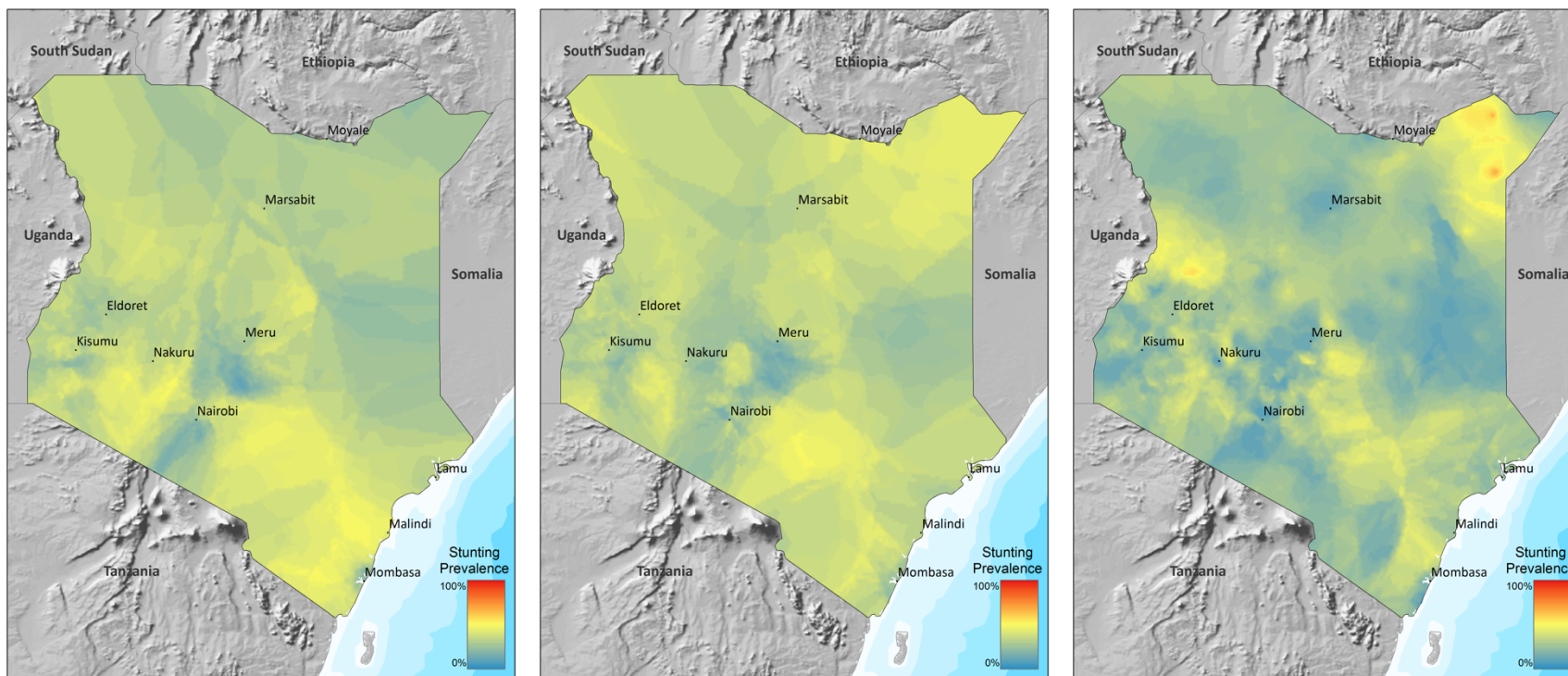


Figure 15: Empirical Bayesian kriging of sample stunting prevalence across Kenya in 2003, 2008 and 2014 DHS surveys.

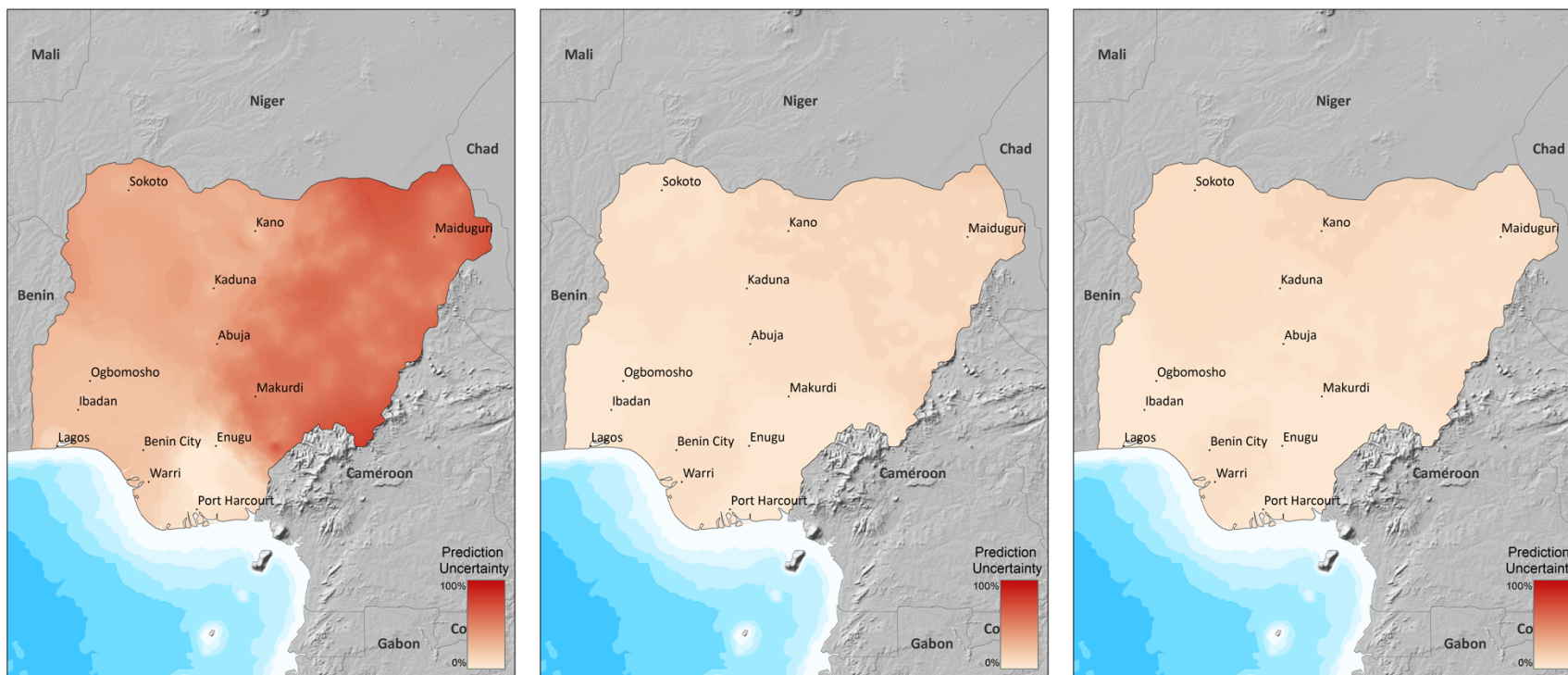


Figure 16: Empirical Bayesian kriging model uncertainty estimates of sample wasting prevalence across Nigeria in 2003, 2008 and 2013 DHS surveys.

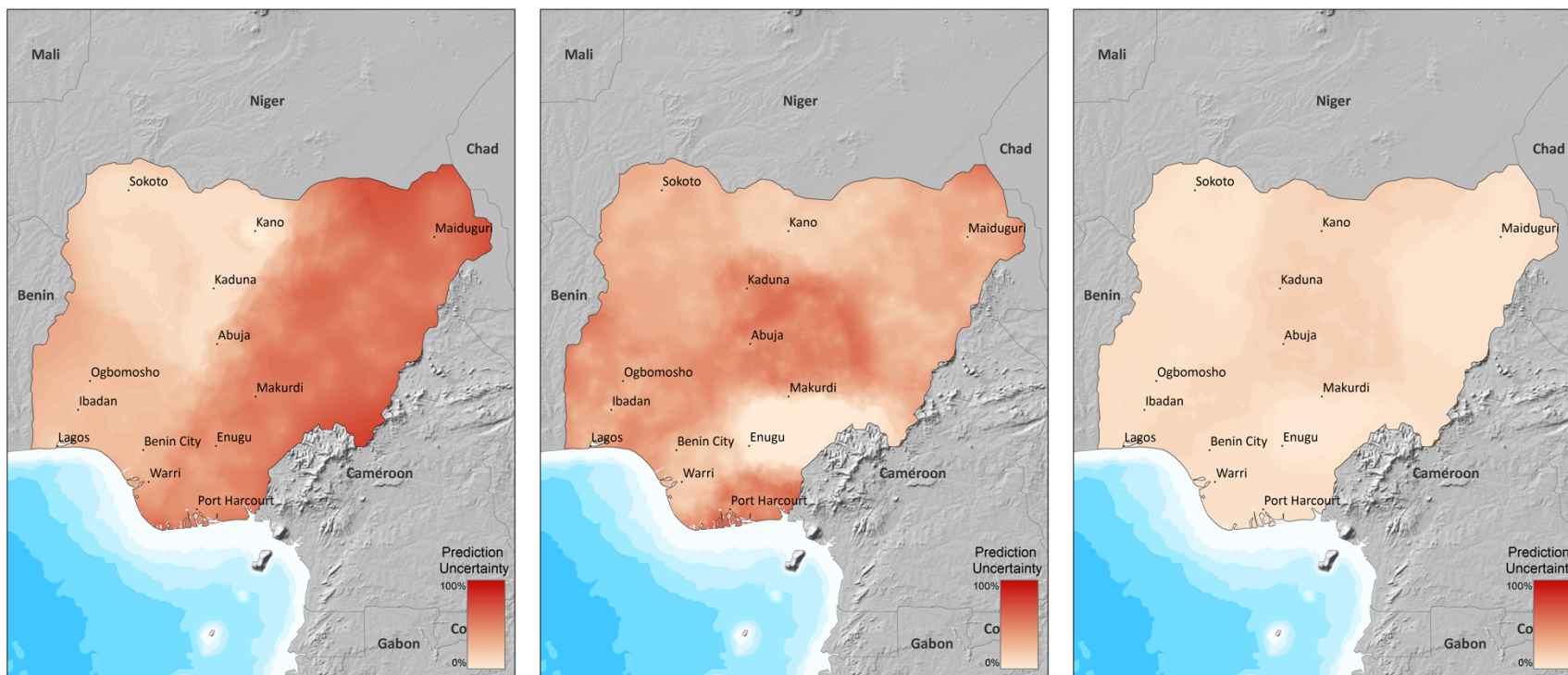


Figure 17: Empirical Bayesian kriging model uncertainty estimates of sample stunting prevalence across Nigeria in 2003, 2008 and 2013 DHS surveys.

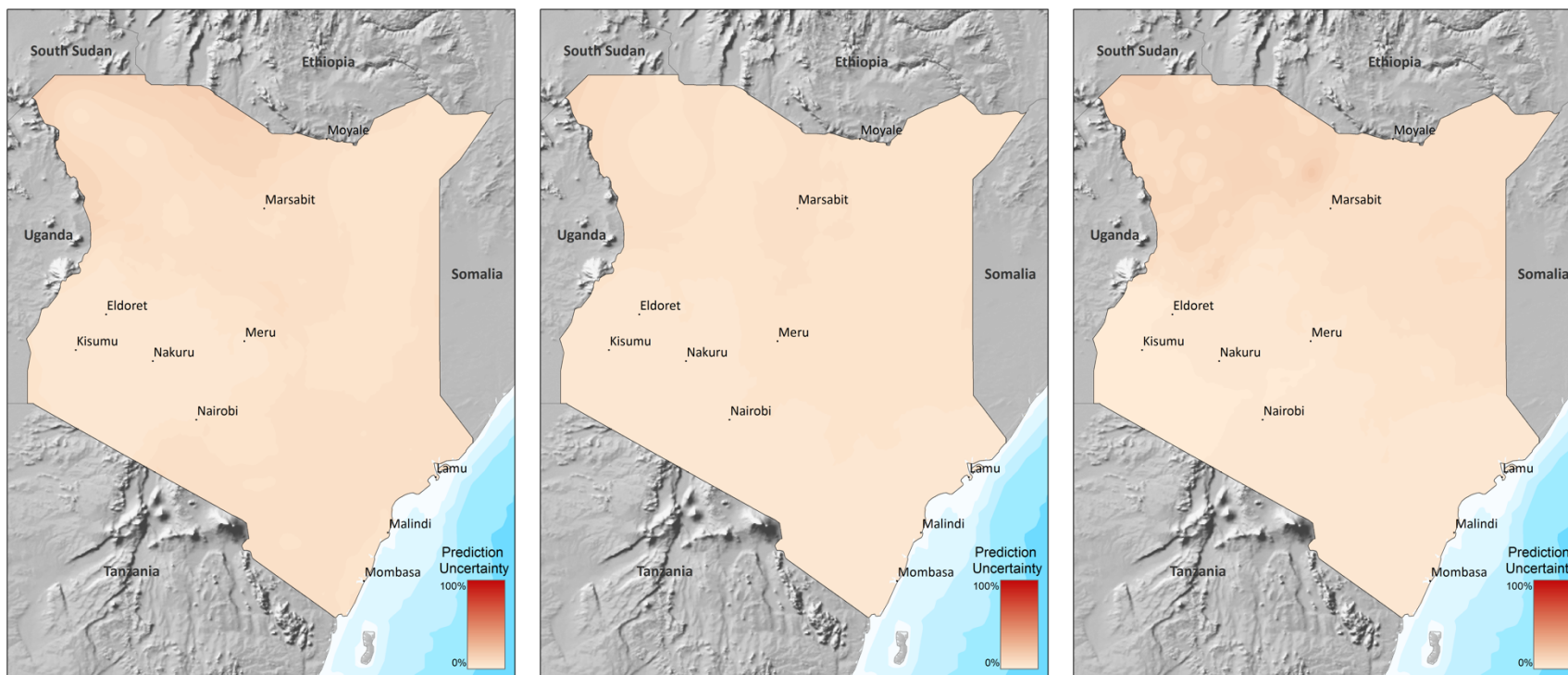


Figure 18: Empirical Bayesian kriging model uncertainty estimates of sample wasting prevalence across Kenya in 2003, 2008 and 2014 DHS surveys.

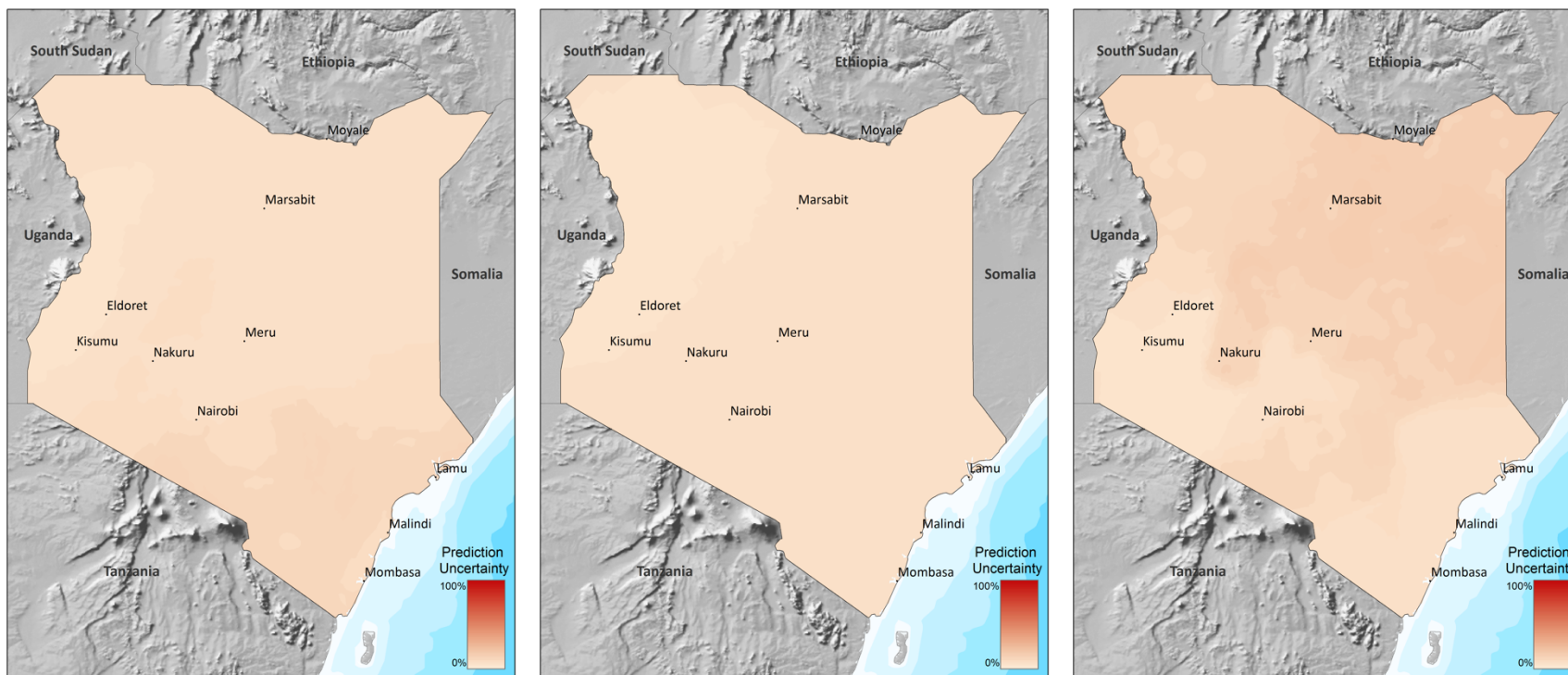


Figure 19: Empirical Bayesian kriging model uncertainty estimates of sample stunting prevalence across Kenya in 2003, 2008 and 2014 DHS surveys.

7.3.5 Econometric Methodology

7.3.5.1 Motivating Principles

The term *regression* can refer to a wide range of procedures, which model the relationship between a dependent outcome variable (e.g., malnutrition), and a set of independent regressors or latent determinants. Let Y_i denote the dependent anthropometric variable and X_{ip} denote the independent latent determinants, where $i = 1, 2, \dots, n$ indexes the sample of children and $p = 1, 2, \dots, P$ indexes the different regressors. The outcome variable is binary so that $Y_i = 0$ if child i is healthy (z-score is greater than or equal to negative two standard deviations from the reference median) and $Y_i = 1$ if child i is malnourished (z-score is less than negative two standard deviations from the reference median). The values of 0 and 1 are arbitrary and chosen for simplicity without any loss of generality.

For exposition, it is convenient to think of a regression as a *conditional prediction* or more precisely a *projection*, which Cameron and Trivedi show can always be pertinent and derivable even if the causal or structural relationship is undefined (Cameron & Trivedi, 2005). Let \hat{Y}_i denote the *predictor* defined as a function of X_{ip} and let $e_i \equiv Y_i - \hat{Y}_i$ denote the *prediction error* so that $L(e_i) = L(Y_i - \hat{Y}_i)$ denotes the *loss* associated with random error. The *optimal predictor* minimizes the expected loss.

The specific functional form of the loss function should depend substantively on the losses associated with prediction errors. Implicitly in most econometric applications the loss function is quadratic, such that $L(e_i) = e_i^2$ and $E(Y_i|X_{ip})$ is the

optimal predictor (Cameron & Trivedi, 2005). But the convention of a quadratic loss function has its basis in convenience and not substance.

A regression models the distribution of anthropometric malnutrition (Y_i) corresponding to a fixed level in the variables that determine malnutrition (X_i), for example wealth or plant vitality. In other words, it gives the *conditional distribution* of Y_i conditional upon the given values of X_i . The *conditional expectation* summarizes the conditional distribution relationship, denoted by $E(Y_i|X_{ip})$, or plainly the expected value of Y given the specific values of X_{ip} . Because the dependent variable of malnutrition is binary, the conditional expectation also corresponds to the *conditional probability*, denoted as $E(Y_i|X_{ip}) = \Pr(Y_i = 1|X_{ip}) = \mathcal{P}_i$. Because they follow a *Bernoulli model*, binary outcomes are relatively straightforward to model. If the probability of, say, wasting equals \mathcal{P}_i , then the probability of not wasting must be $(1 - \mathcal{P}_i)$. For regression applications the probability \mathcal{P}_i will vary across children as a function of the regressors.

7.3.5.2 Linear Probability Specification

The symbology and explication reveal the conditional expectation and by extension the conditional probability is some function of X_i denoted by $\mathcal{P}_i = f(X_i)$. The specific functional form of $f(X_i)$ is specified by the purpose of the analysis. For the purposes of modeling the latent determinants of malnutrition, suppose the conditional probability function is linear and additively separable so the *linear probability model* specification takes the form

$$Y_i = \beta_0 + \sum_{p=1}^P \beta_p X_{ip} + e_i,$$

where, X_{ip} for $p = 1, 2, \dots, P$ is the set of explanatory variables, and e_i is the unobservable stochastic error term. Since the relationships are not deterministic, the error term is conceptualized as the random deviation of a child's expected outcome. The coefficients, β_0 and β_p , are unknown but fixed parameters that quantify the relationship between the explanatory variables and the malnutrition. Again, because the dependent variable of malnutrition is binary, one can interpret the regression coefficient β_p as the change in the probability a child is malnourished associated with a change in X_p , holding all other regressors constant.

The objective of regression analysis is to estimate the values of the coefficients. Specifically, of interest is measuring the *marginal effect* (i.e., $\partial P / \partial X_p$) of the latent determinants or plainly the change in the conditional probability of Y_i for a given change in X_p . In a linear probability model the coefficient estimates (β_p) are equivalent to the marginal effects. The interpretation of a marginal effect is consistent across specifications, however, the equivalence of coefficients to marginal effects does not hold for the other model specifications.

Marginal effects have different interpretations depending on whether they are from discrete or continuous variables. For a discrete change in a binary independent variable (i.e., getting a vaccine), the marginal effect measures how much the predicted probabilities (i.e., likelihood a child is wasted) will change in response. For a continuous independent variable, the marginal effect measures an instantaneous rate of change (i.e., how much would a 1-unit change in rainfall impact the likelihood a

child is wasted). Analyzing how much predicted probabilities are likely to change in response to perturbations is insightful for measuring the impact and extent of latent determinants of childhood malnutrition.

Forecasting, which is by definition an out-of-sample process, should not be confused (but often is muddled) with the “predictions” of internally valid marginal effects. It may seem a pedantic distinction, but the what-if scenarios of marginal effects are established by a sturdy, internally consistent, and impelling epistemology.

To begin to establish a causal inference. One must specify and satisfy a set of underlying assumptions about the model and data generation process, what Goldberger refers to as a *structural model* (Goldberger, 1972). Derived from the foundation laid by Gauss (1821):

- (1) The outcome variable and the regressors are independent and *not* identically distributed over \mathbf{i} , which is necessary because of the stratified sampling structure and will affect the asymptotic properties of the estimators.
- (2) The model is correctly specified, which may seem obvious, but is devilishly tricky to ensure since it encompasses linearity, omitted variables, measurement error, and random parameters.
- (3) The regressors have a defined finite second moment (i.e., variance) and have no perfect linear relationship (i.e., multicollinearity).
- (4) The expected value of the errors conditional on the regressors is zero, which in turn implies the errors are uncorrelated with the regressors (i.e., all excluded factors have no impact on the outcome).
- (5) The errors are independent and uncorrelated with the regressors (i.e., the conditional covariance of the errors is zero).
- (6) The number of observations is greater than the number of estimated coefficients.

Making the assumptions of the linear probability model regression explicit is not only a prudent deed in the name of precision; it also emphasizes the need for alternative specifications when the assumptions are violated.

7.3.5.3 *Interlude: A Methodological Rejoinder*

Anthropometry is ineluctably linked to regression analysis. The very term *regression* originates with the titan of anthropometry, Francis Galton (Gujarati, 1995). Galton (1886) observed that, although tall parents beget tall children and short parents beget short children, the average height of children for a given parental height tended to “regress” towards the average height in the population. In a typical eugenic spirit, Galton referred to this phenomenon as “regression to mediocrity.” For better or worse, the terminology and its application has since been reclaimed.

Regression models have many applications, which include: data summary, prescient forecasting, and causal inference, which is my primary modeling task. Causal inference depends on a strict nexus of assumptions regarding data collection, analysis, and presentation; the full set of assumptions is embodied in a model that underpins the method (Greenland et al., 2016). For empirical studies, however, establishing causal inference is like Ahab and his White Whale:

To me the white whale ... tasks me; he heaps me ... with an inscrutable malice. ... All visible objects, ... are but as pasteboard masks. ... [S]ome unknown but still reasoning thing puts forth the mouldings of its features from behind the unreasoning mask. (Melville, 1892, p. 157)

The task of an investigator of scientific truth is to strike through the mask; indeed: Truth hath no confines. And for some outcomes there is no limit to the sum of possible causes (Rothman, 1976; Wensink et al., 2014).

The distinguished mathematical statistician, David A. Freedman, suggests that causal inference is “the most interesting and the most slippery” of the uses of regression models (2005, p. 1). To make matters slipperier, my data is *observational* (as opposed to *experimental*). “When using observational data to make causal inference,” remarks Freedman, “the key problem is *confounding*” (2005, p. 1). “Confounding is the major concern in epidemiological analyses of observational studies” agrees clinical biostatistician Ewout Steyerberg (2009, p. 27). If a hidden factor is associated with the independent variable and influences the dependent variable, then it is *confounding*—causing spurious results.

Experimental designs assume that it is possible and desirable to isolate specific relations between the small set of variables under experimental control. In experimental design, Peter Lunt makes the point that, “The ontological assumptions are a mechanics based on the putative interaction between these relatively independent variables that can be linked to both empirical evidence and theory through operationalization” (Lunt, 2004, p. 563). In contrast, regression designs assume that there are a multitude of indicators in complex interaction that can be measured rather than controlled, elucidating complex structural relations between multiple variables.

The controlled experiment paradigm aligns physical and mechanical ontological assumptions with verifiable epistemological assumptions connecting data and causal inference (Lunt, 2004). Establishing causal inference is more straightforward with experimental data from *randomized controlled experiments* or even *natural experiments*, where differences between treatment and control groups

are random and investigators rely on the random error to control for confounding factors. Whereas in observational studies, treatment and control groups are self-assigned, or at least non-randomly assigned, and investigators simply observe what happens. Indeed, as Freedman notes, “one objective of statistical modeling is to create an analogy, perhaps forced, between an observational study and an experiment” (2006, p. 691).

But don’t be mistaken and think a randomized control trial (RCT) is the only answer or even a good answer. Astronomers only work with observational data, and yet they manage to do good science. Even in the medical and public health fields many vitally important findings exist outside of RCTs. Implementation of a RCT for sudden infant death syndrome (SIDS) would have been an unethical proposition. However, *observation* of a prone sleeping position as a risk factor led to education programs with significant and substantial reduction in SIDS (Mitchell et al., 1991). Deaton (2006) is skeptical about the general usefulness of randomized controlled trials in the context of international aid and development initiatives.

Ziliak and Teather-Posadas suggest “randomization enthusiasts have paid little attention to the ethical issues, economic costs, and theoretical difficulties caused by the so called randomization principle” (2016, p. 1). Angus Deaton and Nancy Cartwright argue:

Contrary to frequent claims in the applied literature, randomization does not equalize everything other than the treatment in the treatment and control groups, it does not automatically deliver a precise estimate of the average treatment effect (ATE), and it does not relieve us of the need to think about (observed or unobserved) covariates. (Deaton & Cartwright, 2018, p. 2)

Similarly, we cannot observe the individual treatment effects, we can only observe their mean, assuming that the mean is a linear operator, such that the difference in means is the mean of differences. Medians, percentiles, or variances of treatment effects, cannot be identified from an RCT (Deaton, 2010). Others note RCTs lack external validity, the ability to assess effect duration, and cannot identify rare but serious adverse effects (e.g., Chavez-MacGregor & Giordano, 2016; Frieden, 2017; Rothwell, 2005).

Similar to evaluating the latent determinants of childhood malnutrition, for example, studies of smoking's effect on one's health are necessarily observational. Freedman explains that, "There is a strong *association* between smoking and disease. Generally, association is circumstantial evidence for causation. However, the proof is incomplete" (2005, p. 2). Association (i.e., correlation) is not the same as causation.

The distinguished epidemiologist, Kenneth J. Rothman warns that the strength of an association (model fit) has little bearing on causality since weak associations can be causal and strong associations can be noncausal.

Strength of association depends on the prevalence of other factors. Some causal associations, such as the association between cigarette smoking and coronary heart disease, are weak. Furthermore, a strong association can be noncausal, a confounded result stemming from the effect of another risk factor... that is highly correlated with the one under study. (Rothman, 2012, p. 33)

To ascribe causality, one must appeal to *a priori* theoretical considerations (Gujarati, 1995). Freedman notes that, "Statisticians like Joseph Berkson and R. A. Fisher did not believe the evidence against cigarettes, and suggested possible confounding variables" (2005, p. 2). Epidemiologists, however, observed that death rates for smokers were higher because cigarettes kill and carefully showed the

possible confounding variables were not plausible. Once more, the task of an investigator is to establish causal inference with careful scrutiny of observational studies and to control for confounding factors.

Many leading observers of mathematical statistics take umbrage with statistical models that embrace unrealistic or unjustified assumptions, such as random sampling or randomization. William Sealy Gosset aka “Student”—of Student’s test of statistical significance—in explicit opposition to R. A. Fisher, ventures to point out “advantages of artificial randomization are usually offset by an increased error” (1938, p. 363). The great scientist and polymath, Harold Jeffreys, agrees, stating that the “hypothesis of the randomness of the residuals, which is needed for the validity of the method of least squares, has nothing to do, intrinsically, with the intended randomness of the original design” (1939, p. 1).

In their guide to misinterpretations, Greenland et al. emphasize that randomization assumptions “are often deceptively simple to write mathematically, yet in practice are difficult to satisfy and verify, as they may depend on successful completion of a long sequence of actions,” which they catalog as “identifying, contacting, obtaining consent from, obtaining cooperation of, and following up subjects, as well as adherence to study protocols for treatment allocation, masking, and data analysis” (2016, p. 338). Nobel Prize-winning economist Angus Deaton (2007) does not believe randomization is a panacea for identification problems in econometrics, calling those who do believe “randomistas.” In their illocutionary work on the randomization principle in economics and medicine, Ziliak and Teather-Posadas (2016) show the “principle” was fabricated out of nothing by R. A. Fisher.

James Heckman (also a Nobel Laureate) and Edward Vytlačil note that “Randomization is a metaphor and not an ideal or ‘gold standard’” (2007, p. 4836). And Deaton and Cartwright warn: “The gold standard or ‘truth’ view does harm when it undermines the obligation of science to reconcile RCTs results with other evidence in a process of cumulative understanding” (2018, p. 5).

In his reprimand of charlatan econometrics, Edward Leamer (1983) points out randomization is only a *necessary* condition of experimental data; it is not *sufficient* to establish causal inference. Leamer even states that, “One should not jump to the conclusion that there is necessarily a substantive difference between drawing inferences from experimental as opposed to nonexperimental data.” He goes on to say “we must resist. ‘Random’ does not mean adequately mixed in *every* sample. Randomization implies that the [estimate] is ‘unbiased,’ but that definitely does not mean that for each sample the estimate is correct” (1983, p. 31). It would be ignorant and insincere of me to rely on unrealistic or unjustified assumptions, which are unnecessary to establish my task of causal inference. I believe the observers of mathematical statistics make an impelling argument.

Many leading observers of mathematical statistics will also point out observational studies are not necessarily even at a loss when it comes to establishing controls. “Why are we not content simply to describe specific parts of the heterogenous world that we see around us, using the rigorous methods of science,” wonders Michael Goodchild, the foremost expert in geographic information science. And he is exasperated by the insistence “instead that inferences be made about some poorly conceived and nonexistent hypothetical world” (2009, p. 415). “The

nonexperimental scientist by definition cannot control the levels of extraneous influences” observes Leamer, which is not to say the experimental scientist is free from extraneous influences. Further, the nonexperimental scientist can control for an extraneous influence (i.e., a confounding factor) by including it as a variable in estimating equation (provided that it is not perfectly collinear with treatment variable). “The collinearity in naturally selected treatment variables may mean that the data evidence is weak,” notes Leamer, “but it does not invalidate in any way the [effect of the] estimates. Here, again, there is no essential difference between experimental and nonexperimental inference” (1983, p. 34).

There are two competing epistemologies of econometric analysis: fit versus oomph. The fit approach desires to explain as much of the variation in malnutrition as possible (i.e., high precision) regardless of theoretical or practical implications. If the hem length of a mother’s skirt or which end of an egg a person cracks improves fit, then it should be included in the analysis, argue the high fit camp, regardless of whether or not in Truth the covariate actually has an effect on malnutrition. And it is tempting to value fit and precision above all else, since we can never really know the Truth. Marketers and financial analysts value fit, but the students of Science value oomph.

In their monograph on the subject, John Aldrich and Forrest Nelson contend, “Regression may be one of the most abused statistical techniques in the social sciences” (1984, p. 9). Of particular relevance to this research they warn, “use of the coefficient of determination [i.e., measurement of fit] as a summary statistic should be avoided in models with qualitative dependent variables” (1984, p. 15).

As the illustrious economist, historian, and rhetorician Deirdre McCloskey asserts, “What matters is oomph. Oomph is what we seek.” She goes on to note, “Statistical significance, which now guides a large part of the intellectual life of economists, has nothing to do with oomph” (1986, p. 5). Even by 1939, as McCloskey annotates, the *Statistical Dictionary of Terms and Symbols* stated plainly: “A significant difference is not necessarily large, since, in large samples, even a small difference may prove to me a significant difference. Further, the existence of a significant difference may or may not be of practical significance” (Kurtz & Edgerton, 1939; 1985, p. 203).

Given my samples are stratified and relatively large, I do not have a genuine worry about a sampling error of excessive skepticism, but instead should be worried about more significant sources of error, such as confounding effects, specification error, non-linear fertility slopes, the bias of the auspices, measurement error, experimental error, sample selection bias, efficiency, consistency, misclassification, endogeneity, heterogeneity, heteroskedasticity, multicollinearity, idiosyncratic error, specification error, and functional form (see Ziliak & McCloskey, 2008).

The problem of pernicious p -values is not new (Berkson, 1942; Boring, 1919; Neyman & Pearson, 1928; Student, 1908a, 1908b, 1927) nor relegated to the fringe of contrarian publications (Bruns & Ioannidis, 2016; Cohen, 1990, 1994; Freiman et al., 1978; Leamer, 1983; McCloskey, 1985; McCloskey & Ziliak, 1996; Nuzzo, 2014; Rothman, 1978; Siegfried, 2010) nor a notion incapable of orthodox reverence (Fidler et al., 2004; Ioannidis, 2005; Rothman, 1998; Shrout, 1997; Sullivan & Feinn, 2012;

Wasserstein & Lazar, 2016; Wasserstein et al., 2019), yet the problem persists at full tilt.

Of the 184 disaggregate empirical studies of the determinants of childhood malnutrition in Africa since 1990 that I scrutinized for review—undoubtedly representing a supermajority of the highest quality scientific literature on the topic—98% of studies mistakenly rely only on statistical significance to ascertain the importance of the determinants. A good sign for potential future discoveries from researchers aptly employing what Goodchild (2009) and many others call the “rigorous methods of science” and a bad sign for the millions of children who continue to suffer because we, as a scientific community, continue to be satisfied with fooling ourselves.

Roger E. Kirk, distinguished professor of psychology and statistics, highlights three major criticisms of statistical significance. The first and most blatant is the fact that “significance testing and scientific inference address different questions” (Kirk, 1996, p. 747). In effect, statistical significance does not answer the question researchers are asking. The empirical studies that mistakenly rely only on statistical significance are guilty of *the fallacy of the transposed conditional*. The mistaken studies claim to observe the likelihood of a null hypothesis assuming the data are true, gleaned from what they actually test, which is the likelihood of the data assuming the null hypothesis is true. No. It is wrong to assume $\Pr(\text{Data}|\text{H}_0) = \Pr(\text{H}_0|\text{Data})$.

Even falsification of H_0 implies either that the hypothesis is wrong or that any number of tacit variables, side conditions, or alternative hypotheses $\text{H}_1, \text{H}_2, \text{H}_3, \dots \text{H}_n$ intervened (Ziliak & McCloskey, 2008). Furthermore, Kirk explains it is also wrong

to believe “the p -value is the probability that the null hypothesis is correct, and the complement of the p -value is the probability that a significant result will be found in replication” (1996, p. 747). Distinguished epidemiologist and longtime decrier of statistical abuse, Steven Goodman, is convinced that the

most serious consequence of this array of p -values misconceptions is the false belief that the probability of a conclusion being in error can be calculated from the data in a single experiment without reference to external evidence or the plausibility of the underlying mechanism. (2008, p. 135)

The second criticism is that statistical significance is trivial. As the profound mathematician and statistician John Tukey explains, “the effects of A and B are always different—in some decimal place—for any A and B. Thus asking “Are the effects different?” is foolish” (1991, p. 100). And Tukey drives the point by explaining that statisticians are not only asking the wrong question, but are lying if they are willing to answer no. One can always reject a hypothesis given a large enough sample and one can always fail to reject a hypothesis given a large enough precision. “You cannot “test” mechanically for nonzero along some scale that has no dimension of substance and cost” state Ziliak and McCloskey; they insist that “Real scientific tests are always a matter of how close to large or how close to some parameter value, and the standard of how close must be a substantive one, inclusive of tolerable loss” (2008, p. 98). Kirk laments the irony that

a ritualistic adherence to null hypothesis significance testing has led researchers to focus on controlling the *Type I error* (false positives) that cannot occur because all null hypothesis are false while allowing the *Type II error* (false negatives) that can occur to exceed acceptable levels. (1996, p. 747)

If I were to offer you a cup of tea, you would probably want to know how hot it was before you drank it: a meaningful quantitative question. But if you were to rely

only on statistical significance to make that decision—just as too many studies have—you would only really learn whether or not the tea is in fact exactly zero degrees or not. Interesting, maybe, but not informative. Statistical significance can only answer the qualitative question: Is the temperature of my tea exactly zero or not? Even if the answer is measured imprecisely, the more informative and relevant question is, “How hot is my tea?”

The third criticism is that statistical significance testing profligates a continuum of uncertainty into a dichotomous decision. “Statistical significance is not a *scientific* test,” note Ziliak and McCloskey, “It is a philosophical, qualitative test. It does not ask how much it asks “whether”” (2008, pp. 4-5). By his own admission we know that Fisher’s “rule of 2” (i.e., $p = 0.05$; i.e., a 1 in 20 chance), is not a universal transcendental truth, but merely a matter of convenience (Fisher, 1925, 1926, 1935). Or as Rosenthal and Rubin would put it, “Surely, God loves the 0.06 nearly as much as the 0.05” (1989, p. 1227). The philosophical, qualitative, dichotomization of science further leads to the misconception that failure to reject the null hypothesis is evidence for accepting it. No. “A more refined goal of statistical analysis” notes Greenland et al. “is to provide an evaluation of certainty or uncertainty regarding the size of an effect” (2016, p. 339).

The oomph approach values the impactfulness of a variable (i.e., a large coefficient) within the setting of an impelling, persuasive story, (i.e., theoretical model) as to why the variable would actually matter (i.e., practical usefulness) even if the variable is imprecisely measured (i.e., large standard errors or low fit). Students of Science have long understood that precision is nice, but oomph is essential (Ziliak &

McCloskey, 2004a). Or as Tukey proclaims, “Empirical knowledge is always fuzzy! And theoretical knowledge...is always wrong-in detail, though possibly providing some very good approximations indeed” (1991, p. 101).

The point, note Ziliak and McCloskey (2008), has been reiterated by Edgeworth, Gosset, Egon Pearson, Jeffreys, Borel, Neyman, Wald, Wolfowitz, Yule, Deming, Yates, L. J. Savage, de Finetti, Good, Lindley, Feynman, Lehmann, DeGroot, Bernardo, Chernoff, Raiffa, Arrow, Blackwell, Friedman, Mosteller, Kruskal, Mandelbrot, Wallis, Roberts, Granger, Leamer, Press, Moore, Berger, Gigerenzer, Freedman, Rothman, and Zellner. As such my scientific paradigm is oomph—a tradition with historical exemplars (Kuhn, 1977).

The purpose of the analysis specifies its paradigm and defines what is of value. In the clinical setting of public health and epidemiology, a *diagnostic* application helps to estimate the probability that malnutrition is present, identifying the nature or cause of the malnutrition; whereas a *prognostic* application helps to predict how malnutrition will develop and target preventive interventions to children at relatively high risk. Diagnostics can be described as the probability of malnutrition conditional on a set of latent determinants, whereas prognostics can be thought of as the obverse or the probability of future outcomes conditional on being malnourished. From a study design perspective, prognostic studies are inherently longitudinal in nature, whereas diagnostic studies are most often cross-sectional (Steyerberg, 2009). The terminology is easily muddled since the predictive characteristics in diagnostic studies relate to an underlying diagnosis.

Estimated effects from latent determinants provide the diagnostic insights. An assiduous diagnostic study examines a well-defined cohort of children suspected of being malnourished, where the outcome is the underlying diagnosis and several covariates may simultaneously be latent determinants. Harm versus benefit establishes the prognostic framework. The purpose of a prognostic model is that better decisions are made with the model than without. Within the prognostic framework reliability of predictions is key. It is my purpose to estimate the probability of malnutrition in a diagnostic sense, and to help target preventive interventions in a prognostic sense, though strictly speaking my study is not prognostic.

There is a further competition of modeling epistemologies between *prescient forecasting* and *causal inference*. Much like fit versus oomph, prescient forecasting is tempting, but theoretically licentious; whereas causal inference is more ephemeral and precious, but scientifically motivated. Prediction is an oft misunderstood and abused concept. We cannot know the future; haruspex remain ineffectual. Indeed, economists can forecast business cycle peaks and are generally correct in their predictions, but generally a good deal out in their dates with lead times ranging from 1 to 19 months (McCloskey, 1992). Such a wide lead time is little better than predicting if it's August in southern Florida, then there will be an ensuing hurricane after a while. It is a prediction, but not an economically profitable one. At any rate, it is not nearly valuable enough to short orange juice concentrate futures on the Chicago Stock Exchange for a cheap fortune. Economically profitable predictions are impossible by definition. Pioneering econometrician Halbert White acknowledges

that “Even when no exploitable forecasting relation exists, looking long enough and hard enough at a given set of data will often reveal one or more forecasting models that look good, but are in fact useless” (2000, p. 1097).

Each of us can “predict” that the sun will rise in the east tomorrow morning and even the morning after, but knowing as much does not provide cheap fortunes in economic profits beyond the usual discounted returns that we all possess. With careful observation and technical prowess, it is possible to make similar “predictions” about more obscured phenomena. A meteorologist knows low pressure predicts thunderstorms. A gastroenterologist knows how acute pain in the lower abdomen predicts appendicitis. To the uninitiated, the meteorologist and gastroenterologist seem clairvoyant. Their “predictions” are useful, but not economically profitable nor true prescience. McCloskey notes that “Prescience”:

much like cheap fortunes, is an oxymoron: “Pre-science” is knowing before one knows. ... In human affairs a forecast beyond what earns a usual return is impossible, except by entrepreneurs, idiot savants, *auteurs*, and other prodigies of tacit knowledge. (1992, pp. 35-36)

As such, when I refer to my predictions, predicted probabilities, or predicted effects, I hope it is painfully clear that I do so in an inferential, diagnostic sense and not a prescient one.

Clinical prediction models often use *decision analysis* to support models that estimate the probability of an underlying disease (e.g., malnutrition)(Steyerberg, 2009). The methodology of decision analysis formally weighs the costs and benefits of a decision using a *treatment threshold* and *loss function*. The threshold demarcates the probability where the expected benefit from treatment is equal to the expected benefit of avoiding treatment. The relative weight of false-negative vs. false-positive

decisions determines the threshold (Steyerberg, 2009). Data dredging is remains a major problem, contend Smith and Ebrahim:

When a large number of associations can be looked at in a dataset where only a few real associations exist, a P value of 0.05 is compatible with the large majority of findings still being false positives. These false positive findings are the true products of data dredging, resulting from simply looking at too many possible associations.... As with bias, increasing the significance level provides no protection against being misled by confounded associations. (Smith & Ebrahim, 2002, p. 1437)

Clinical prediction models are useful when the diagnosis is sufficiently uncertain for effective decision-making. One source of uncertainty is from measurements of malnutrition with error—*sensitivity* or *specificity* below 100%. Among the sample of possibly malnourished children, sensitivity is the fraction of true-positives, and specificity is the fraction of true-negatives. Unfortunately, it is common in diagnostic evaluations to have misclassification error in the predictive characteristics and outcome assessments. Avenues for error, which dilute the association of predictors with malnutrition, include *observer variability* and *biological variability*. And misclassification error in the outcome variable causes inconsistent coefficient estimates in discrete-response models (Hausman et al., 1998; Sandler & Rashford, 2018).

For practical diagnostic identification and comparative prognostic usefulness, ideally a *gold standard* is available where both sensitivity and specificity are 100%. A gold standard is definitive, but may not be available, suitable, practical, or even exist at all. The very phrase *gold standard* is itself equally revealing. It is amusing that just as the U.S. government abandoned the dubious gold standard financial practice in the 1970s, the medical community adopted the term for clinical, diagnostic, and treatment

“best practices” and in particular for randomized controlled trials (Jones & Podolsky, 2015).

To classify a child as malnourished or not, one must apply a cutoff value to the predicted probability. It is common to use 50% as the cutoff, but it is not defensible in a medical context (Steyerberg, 2009). It implies false-positive and false-negative classifications are equally important. Instead, I employ the loss-function to maximize net benefit, by examining the sensitivity and specificity values over the entire range of cutoffs (0% to 100%). The clinical usefulness of the model is measured by the gap between the predicted outcome and the actual outcome.

Remember, the purpose of my analysis is to evaluate the latent determinants that impact the severity and variability of childhood malnutrition. The eminent econometrician, Jeffrey Wooldridge (2010), asserts that determining the change in one variable caused by another variable is the goal of most empirical studies and at the crux of establishing that causal relationship is the notion of *ceteris paribus*, that is, holding all other relevant factors fixed. Now, it would be impractical—not to mention unethical—to run a controlled experiment to uncover the causality of malnutrition in young children (see Ziliak & Teather-Posadas, 2016); so instead, I use econometric methods to effectively hold all other relevant factors fixed.

But the equivocal question then arises of which method to use among a staggering plethora of choices. In truth, choosing which methodology to use and which is most effective is not a positivistic, operationalistic, or dialectic endeavor, but a rhetorical one (McCloskey, 1983). Or as the philosopher of science Richard Rorty puts it, “scientific breakthroughs are not so much a matter of deciding which of

various alternative hypotheses are true, but of finding the right jargon in which to frame hypotheses in the first place” (1982, p. 193). The Nobel Laureate and prodigy, Kenneth Arrow, evaluated the soundness of competing theories based on *persuasiveness*. He asked of a theory, “Does it correspond to our understanding of the economic world? ... If you find a new concept, the question is, does it illuminate your perception? Do you feel you understand what is going on in everyday life?” irrespective of fit (Feiwel, 1987). Indeed, as the pioneering econometrician Edward Leamer proclaims, “Models are neither true nor false. Models are sometimes useful and sometimes misleading” (2004, p. 555).

As I previously discussed model fit is never the ultimate aim of scientific research. It can be illuminating, but it is only one of many subordinate findings to the central aim of oomph. Fit has an added layer of obscurity in binary outcome models such as these, since the *predicted outcome* (discrete prediction of a child being malnourished or not) is a function of the modeled *predicted probability* (continuous prediction from 0% to 100% of a child being malnourished). The choice of function and its parameters will drive most of the findings. For example, if your only goal was to develop a model with perfect sensitivity (i.e., 100% true positive rate) or the percentage of wasted and stunted children correctly identified as being wasted and stunted, then the modeling task is trivial. Simply take all observed children and assign each one a predicted probability of one, no other inputs required. You will never mistakenly predict that a malnourished child is actually healthy. Obviously, actually implementing such a model is absurd, but it helps to illustrate the point. The point being that without a loss function the clinical usefulness of a model is moot.

Again, to classify a child as malnourished or not, one must apply a cutoff value to the predicted probability. It is common to use 50% as the cutoff, but it is not defensible in a medical context (Steyerberg, 2009). It implies false-positive and false-negative classifications are equally important. Instead, I employ the loss-function to maximize net benefit, by examining the sensitivity and specificity values over the entire range of cutoffs (0% to 100%). The clinical usefulness of the model is measured by the gap between the predicted outcome and the actual outcome. The cutoff values for the hierarchical model specification from the decision curve analysis are an average 15.3% for Nigeria wasting; 4.5% for Kenya wasting; 38.7% for Nigeria stunting; and 31.1% for Kenya stunting. The subsequent measures of sensitivity (true-positive rate) and specificity (true-negative rate) under the maximized net benefit regime range from 77.2% at a minimum to a maximum of 95.3% with a value of 84.2% on average: indicating a good fit.

I also calculate other measures of fit such as *percent correctly classified*, *McIntosh-Dorfman criterion*, and *McFadden's pseudo-R-squared*. If the predicted probability is at least .5 (or .15 as the case may be under a maximized net benefit decision curve analysis), then the predicted outcome takes a value of one, and zero otherwise. The percent correctly classified measure is the percentage of times each pair of predicted outcomes and observed outcomes match; either when both are zero or one. The McIntosh-Dorfman criterion is similar to the percent correctly classified, but ranges between zero and two, where a value greater than one indicates a good fit. It is calculated by adding up the fraction of correctly predicted zeros and the fraction of correctly predicted ones. Unlike the percent correctly classified measure, the

McIntosh-Dorfman criterion would indicate that a predicted outcome function that only returns ones (100% sensitivity and 0% specificity) is not a good fitting model. Bounded between zero and one, McFadden's pseudo-R-squared measure is given by $1 - \mathcal{L}_a / \mathcal{L}_o$, where \mathcal{L}_a is the log-likelihood value from the estimated model, and \mathcal{L}_o is the log-likelihood value from a model with only an intercept.

Although I, too, will add my voice to what many scholars (much more qualified than myself) have echoed *ad infinitum*, that not only are tests of fit often subordinate, superfluous, and misused, there is no single best measure of fit, either (Amemiya, 1981; Cameron & Trivedi, 2005; Cohen et al., 2003; Cramer, 1999; Greene, 2012; Gujarati, 1995; Kennedy, 2003; Maddala, 1983; McFadden, 1974; McIntosh & Dorfman, 1992; Steyerberg, 2009; Train, 2009; Wooldridge, 2010; Ziliak & McCloskey, 2008). Indeed, one does not select the maximum likelihood estimator, which is the basis for many of the measures of fit (e.g., pseudo-R-squared), so as to maximize fit, but rather one selects a maximum likelihood estimator to maximize the joint density of the observed dependent variables (Greene, 2012). Wooldridge aptly notes that as a goodness-of-fit measure, percent correctly predicted is misleading; "In particular, it is possible to get rather high percentages correctly predicted even when the least likely outcome is very poorly predicted," and getting to the heart of the matter he affirms that, "goodness of fit is not as important as statistical and economic significance of the explanatory variables" (2010, pp. 574-575).

7.3.5.4 *Logit Specification*

Again, a linear probability model specification is useful to motivate the initial intuitions and coefficient interpretations, but it ignores the discreteness of the

dependent variable and does not necessarily constrain the predicted probabilities between zero and one. A more appropriate specification is a discrete choice model, which ensures that the probabilities are bounded between zero and one (i.e., $0 < \mathcal{P}_i < 1$). A *logit model* is by far the easiest and most widely used discrete choice model due to the closed form of the choice probabilities and readily interpretable results (Train, 2009). First described by Pierre-François Verhulst (1845), a pupil of Quételet, the *logistic function* is given by

$$\mathcal{P}_i = \Pr(Y_i = 1 | X = X_i) = \frac{e^{\beta_0 + \sum_{p=1}^P \beta_p X_{ip}}}{1 + e^{\beta_0 + \sum_{p=1}^P \beta_p X_{ip}}}.$$

The function traces a sigmoid curve in which \mathcal{P}_i rises monotonically between zero and one, and the rate varies according to the definition of the variables (Cramer, 2002). Since the regression includes an intercept term (β_0), the average in-sample predicted probability necessarily equals the sample frequency (Cameron & Trivedi, 2005). Alternatively, for estimation purposes one can transform the probability function into an *odds ratio* or *relative risk*, given by

$$\frac{\mathcal{P}_i}{1 - \mathcal{P}_i} = e^{\beta_0 + \sum_{p=1}^P \beta_p X_{ip}},$$

which represents the odds a child will be malnourished given their exposure to a set of latent determinants. A further transformation by taking the natural log gives the *log odds ratio*, denoted as

$$L_i = \ln\left(\frac{\mathcal{P}_i}{1 - \mathcal{P}_i}\right) = \beta_0 + \sum_{p=1}^P \beta_p X_{ip} + e_i,$$

which for estimation purposes is linear in the regressors (X_{ip}). Although some in the statistics and epidemiology literature interpret their coefficients in terms of a marginal

effect on the odds ratio or even log odds ratio, I use the marginal effect on the probabilities for their intuitiveness and clarity. Unlike the linear probability model where the marginal effect of X_p is given by β_p the marginal effect of X_p in the logit model is given by

$$\frac{\partial \mathcal{P}_i}{\partial X_{ip}} = \mathcal{P}_i(1 - \mathcal{P}_i)\beta_p.$$

Because the conditional probability that a child is malnourished \mathcal{P}_i is conditional on X_{ip} , the value of the marginal effects change based on the point of evaluation of X_{ip} . Generally, it is best to use the sample average of the observation-wise marginal effects (Cameron & Trivedi, 2005). Otherwise known as the *average marginal effect* or *estimated prevalence difference*, it is given by

$$n^{-1} \sum_{i=1}^n \mathcal{P}_i(1 - \mathcal{P}_i)\beta_p.$$

7.3.5.5 Hierarchical Modeling Motivation

Hierarchical models address the interdependency explicitly and use it as an advantage. In other modeling frameworks, such interdependencies violate necessary underlying assumptions and are a hindrance. For example, variables affected by national policymakers are endogenous at the national level, but are exogenous to children's health at a household level (Smith & Haddad, 2000). Inherently, child malnutrition is an individual and household-level phenomenon, yet it is at the country (and subnational) levels that many policy decisions are made. Using average data can be misleading if distribution is important and differs across countries and conclusions

from cross-national data may not be applicable to individual countries' situations (Smith & Haddad, 2000).

One advantage of hierarchical modeling is the careful and explicit consideration of the units of observation at different levels. With a hierarchical modeling structure, one can specify and measure the variability associated with each level—child, household, cluster, and state—to match the Demographic and Health Surveys data structure. I assume each level is a pure hierarchical set, such that all clusters are contained within one and only one state, all households are contained within one and only one cluster, and all children are contained within one and only one household. The assumption is for modeling specificity and one can safely assume the set structure holds in reality, too. Variables at each level explain the measurement variability and its effect on malnutrition. Effects may also vary randomly among the units at higher levels (i.e., *cross-level variability*). For example, the magnitude of the effect of a child's gender on their probability of being wasted may depend on cluster level characteristics, such as easy access to an improved toilet. Random variability may also exist at the household, cluster, or state scale—implying random intercepts. Explicit formulation of a hierarchical model with effects at, within, and between levels ameliorates issues of impoverished conceptualization (Raudenbush & Bryk, 2002).

Specifically, hierarchical models provide improved estimation of effects within individual units, formulation and testing of hypotheses of cross-level effects, and partitioning of variance and covariance components among levels. Hierarchical models respect the heterogeneity of social experience (Paterson & Goldstein, 1991).

To understand the latent determinants of malnutrition, one must confront how the effects change across and between scales. Each child, household, cluster, and state has its own distinctive variation and characteristics. Understanding how the distinctiveness of location effects malnutrition provides clarity to a dire situation.

Economic geography and spatial economic models have played an essential role in determining the nature of hierarchical structures as far back as 1826 with von Thünen's foundational volume *The Isolated State* (Samuelson, 1983). Studies of urban and regional science envisage hierarchies of cities containing communities, regions containing cities, and countries containing regions (Corrado & Fingleton, 2010).

The error of *aggregation bias* occurs when a variable has a different meaning, and thus a different effect, at different hierarchical levels. For example, the average quality of water and sanitation in a cluster may have an effect on a child's health above and beyond the effect of an individual child's water and sanitation circumstances at home. Hierarchical models alleviate confounding effects by partitioning the effect of water and sanitation quality on health into separate components.

The error of *misestimating precision* occurs in standard error estimates if the model fails to account for dependence among individual responses within a group. Once the grouping has been established, even if it is established at random, the group itself will tend to become differentiated (Corrado & Fingleton, 2010). The group and its members can both influence and be influenced by the composition of the group (Goldstein, 1998). Continuing from the previous example, the survey design may

have selected the survey clusters at random yet the composition of children within a cluster is likely interdependent. An individual child's water and sanitation circumstance is reliant on the available infrastructure and cultural conventions of that child's community and so, too, is the child living next door, but far less so is the child living five states away. Hierarchical models alleviate derelict dependence by providing a unique random effect for each organizational unit. The standard error estimates incorporate the variability of the random effects, or in survey research terminology, they adjust for *intraclass correlation* (Raudenbush & Bryk, 2002).

Different data come from different organizational levels depending on their *unit-of-analysis*: Local, regional, global; personal, familial, communal; or organelle, cell, tissue, organ, organism, population, ecosystem, biosphere. Hierarchical modeling is special because the *units-of-analysis* are preserved across levels in a combined structure. Inference about the nature of an individual, deduced from inference of the group to which the individual belongs, is known as the *ecological fallacy*. If you observe that some countries in sub-Saharan Africa have a high prevalence of malnutrition, you would fall victim to the *ecological fallacy* concluding that, therefore, if an individual lives in sub-Saharan Africa, they must be malnourished. Or conversely, *emergent properties* of a group cannot be inferred from its constituent part (Mill, 1843). The obverse is known as the *atomistic fallacy*, where associations found at the individual level are assumed to hold for the group as a whole. Hierarchical modeling avoids these fallacies by considering all levels simultaneously (Roux, 2002).

Effective hierarchical models use the entire assemblage of data across each organizational level to provide separate predicted probabilities for each category of interest. The estimators are weighted composites from the category of interest and the overall sample. *Within group* units are more similar than *between group* units and across levels, which mimics *the first law of geography*—everything is related to everything else, but near things are more related than distant things (Tobler, 1970). Children within the same household tend to be more similar to each other than those in other households, similarly for households within clusters, and clusters within states, and even for children within clusters, and households within states. One reason for this is that clustering occurs through some mechanism interconnected to unit characteristics (e.g., a family or a community). Siblings do not end up in the same household by random chance.

There are many guises of hierarchical models across different disciplines. For example, they are called *multilevel linear models* in sociology; *mixed-effects models* and *random-effects models* in biometrics; *random-coefficient regression models* in econometrics; and *covariance components models* and *generalized linear mixed models* (GLMMs) in statistics (Grace et al., 2016; Raudenbush & Bryk, 2002). To be as clear, concise, and precise as possible, I abide by the designation of *hierarchical models*. Various disciplines recognize hierarchical models provide clarity and precision.

Researchers have long used cross-level models as a hierarchical simulacrum, where the individual level outcome is a function of both individual and higher group

level variables (Hofmann & Gavin, 1998). For example, a common specification for a cross-level regression is given by

$$Y_{ij} = \beta_0 + \beta_1 X_{ij} + \beta_2 G_j + \beta_3 X_{ij} G_j + e_{ij}$$

where Y_{ij} is the individual level outcome, X_{ij} is the individual level explanatory variable, G_j is the group level explanatory variable, such that everyone in the same group has the same value, and e_{ij} is the unobservable stochastic error term. The coefficient β_2 is the effect of group level explanatory variable on the individual level outcome. The coefficient, β_3 indicates how much effect of the individual level variable, X_{ij} , on the outcome, Y_{ij} , varies across groups, G_j .

However, given this specification and knowledge of the data generating process, the assumptions of ordinary regression techniques I outlined earlier are likely violated, and the model is misspecified (Rabe-Hesketh & Skrondal, 2008).

Individuals within the same group are all perfectly correlated with respect to the group level variable. As such, the covariance between any two error terms within a group are likely to be non-zero, violating the assumption of no serial correlation (see above assumption 4). As Raudenbush and Bryk (2002) describe, a portion of the random error is group random error, which is constant across individuals within a given group. In addition, the conditional variances of the error terms are likely to vary across groups, further violating the stronger restrictive assumption of homoscedastic errors. Statistical procedures often violate the important assumption of uncorrelated error, fortunately hierarchical models correctly account for the error structure (Garson, 2012).

A hierarchical model provides appropriate generalization of the equation to account for differences across groups (Paterson & Goldstein, 1991). Generalization of classical regression methods with hierarchical methods is almost always an improvement in terms of fit, prediction and inference (Gelman, 2006). Instead, one could specify a hierarchical regression for the same phenomenon, given by Individual-level:

$$Y_{ij} = \beta_{0j} + \beta_{1j}X_{ij} + e_{ij},$$

Group-level:

$$\beta_{0j} = \theta_{00} + \theta_{01}G_j + u_{0j},$$

$$\beta_{1j} = \theta_{10} + \theta_{11}G_j + u_{1j},$$

or in single equation mixed form as

$$Y_{ij} = \theta_{00} + \theta_{10}X_{ij} + \theta_{01}G_j + \theta_{11}X_{ij}G_j + u_{0j} + u_{1j}X_{ij} + e_{ij},$$

where β_{1j} and β_{0j} are random individual-level coefficients, and θ_{00} , θ_{10} , θ_{01} , and θ_{11} are group-level *fixed effects*. The last three terms comprise the random error, where e_{ij} is the individual-level error component, u_{0j} is the *random effect* of grouping between groups, and $u_{1j}X_{ij}$ is the random effect of grouping within the group.

One can specify a hierarchical model as a level-by-level model or as a single mixed model. Functionally they are identical, but the level-by-level specification is useful for understanding how and at what level specific covariates enter the model, whereas the mixed model specification is useful for understanding where the fixed effects and the random effects enter the model. In econometric parlance, *mixed-effects models* contain both fixed effects and random effects. The fixed effects are simply estimated directly, analogous to a standard regression. The random effects are not

estimated directly, but are summarized by their estimated variance and covariance structure. Random effects can model random intercepts or random coefficients, and represent various grouping and hierarchical structures.

The modeling framework is easily adaptable to different grouping and random effects structures: suppose $\beta_{1j} = \theta_{10}$ so that it is fixed across group-level units or suppose $\beta_{1j} = \theta_{10} + u_{1j}$ so that it only varies randomly without specifying any predictors for β_{1j} . The framework is also scalable to accommodate more detailed stratification with more meticulous random slopes and intercepts, while maintaining its tractability and intuition, although the requisite specification and computation increase exponentially. Accompanying each additional random component is not only an additional variance parameter, but also an additional covariance component for each pair of random effects. As such, random elements should only be included if theoretically sound and empirically sufficient.

Accompanying the increased complexity is the opportunity to mis-specify the model. For example, Rabe-Hesketh and Skrondal forewarn that “It rarely makes sense to include a random slope if there is no random intercept” (2008, p. 171). And they note “it is seldom sensible to include a random slope without including the corresponding fixed slope because it is strange to allow the slope to vary randomly but constrain its population mean to zero” (2008, p. 171). Similarly, within the hierarchical framework, Rabe-Hesketh and Skrondal conclude, “It is generally not a good idea to include a random coefficient for a covariate that does not vary at a lower level than the random coefficient itself” (2008, p. 172).

For example, it does not make sense to include a district level random slope for the variable *number of hospitals in a district* as it does not vary within the district. Because one cannot estimate the effect of *number of hospitals in a district* for individual districts, it also appears impossible to estimate the variability of the effect between hospitals. Inversely, the issue of low within cluster variance is not much of an issue at all. It does not matter if some of the clusters have insufficient data as long as there are an adequate number that do have sufficient data.

7.3.5.6 Hierarchical Specifications

The first exploratory step in a hierarchical analysis is to estimate an unconditional, intercept-only model. This is the simplest hierarchical model specification, and is fully unconditional, meaning no predictor variables at any level. The purpose of the exercise is to determine the variance components; the unconditional, intercept-only model assumes random effects coefficients have a mean of zero. This procedure is important to discover how variation in wasting and stunting is distributed across the different hierarchies— child, household, cluster, and state. And it provides evidence to justify the application of a hierarchical model in the first place. The variance decomposition shows both the within and between group variability for the proportion of the variance of the outcome.

Child-level:

$$Y_{ijkl} = \beta_{0jkl} + e_{ijkl}$$

Y_{ijkl} : anthropometric indicator (e.g., wasting or stunting) of child i , in household j , in cluster k , in state l .

β_{0jkl} : mean indicator of household j , in cluster k , in state l .

e_{ijkl} : random child effect, deviation of child $ijkl$'s indicator from household jkl 's mean; $\sim N(0, \sigma^2)$.

Household-level:

$$\beta_{0jkl} = \theta_{00kl} + r_{0jkl}$$

θ_{00kl} : mean indicator of cluster k , in state l .

r_{0jkl} : random household effect, deviation of household jkl 's indicator mean from cluster kl 's mean; $\sim N(0, \tau_\beta)$.

Cluster-level:

$$\theta_{00kl} = \eta_{000l} + v_{00kl}$$

η_{000l} : mean indicator of state l .

v_{00kl} : random cluster effect, deviation of cluster kl 's indicator mean from state l 's mean; $\sim N(0, \tau_\theta)$.

State-level:

$$\eta_{000l} = \gamma_{0000} + u_{000l}$$

γ_{0000} : grand indicator mean.

u_{000l} : random state effect, deviation of state l 's indicator mean from grand mean; $\sim N(0, \tau_\eta)$.

The subscripts i, j, k , and l denote children, households, clusters, and states where,

$i = 1, 2, \dots, N_{jkl}$ children within household j , in cluster k , in state l ;

$j = 1, 2, \dots, J_{kl}$ households, within cluster k , in state l ;

$k = 1, 2, \dots, K_l$ clusters, within state l ; and

$l = 1, 2, \dots, L$ states.

The complementary mixed model for the unconditional, intercept-only model is given by,

$$Y_{ijkl} = \gamma_{0000} + u_{000l} + v_{00kl} + r_{0jkl} + e_{ijkl}.$$

The unconditional, intercept-only model is important for estimating the grand mean, γ_{0000} too, and provides information about the outcome variability at each level. Partitioning the variation shows the proportion of variance in the outcome variable that is explained by the grouping structure of the hierarchical model. The variance of the outcome is given by,

$$\text{Var}(Y_{ij}) = \text{Var}(u_{000l} + v_{00kl} + r_{0jkl} + e_{ijkl}) = \tau_{\eta} + \tau_{\theta} + \tau_{\beta} + \sigma^2.$$

Total variability in outcome Y_{ijkl} is partitioned across each level: level 1, σ^2 among children within households; level 2, τ_{β} among households within clusters; level 3, τ_{θ} among clusters within states; and level 4, τ_{η} among sates. The *proportion of variation* attributed to each level is given by,

Level 1:

$$\frac{\sigma^2}{\tau_{\eta} + \tau_{\theta} + \tau_{\beta} + \sigma^2}$$

Level 2:

$$\frac{\tau_{\beta}}{\tau_{\eta} + \tau_{\theta} + \tau_{\beta} + \sigma^2}$$

Level 3:

$$\frac{\tau_{\theta}}{\tau_{\eta} + \tau_{\theta} + \tau_{\beta} + \sigma^2}$$

Level 4:

$$\frac{\tau_{\eta}}{\tau_{\eta} + \tau_{\theta} + \tau_{\beta} + \sigma^2}$$

The *proportion of variation* is related to, but is not the same as, the *intraclass correlation coefficient* measurement, which show the amount of unexplained variation that is attributed to the grouping variable, as compared to the overall unexplained variance (within and between variance). For example, at the household level, an intraclass correlation value of 0.35 would suggest that 35% of the variation in wasting can be explained by which household the child lives in. Intraclass correlation coefficients only apply to random-intercept models (i.e., fully unconditional specification with no predictor variables at any level). The coefficient is often referred to as *rho* and is also known as the *cluster effect* (Raudenbush & Bryk, 2002). For the 4-level fully unconditional hierarchical model the intraclass correlation coefficients are given by,

Level 2:

$$\rho_2 = \frac{\tau_\eta + \tau_\theta + \tau_\beta}{\tau_\eta + \tau_\theta + \tau_\beta + \sigma^2}$$

Level 3:

$$\rho_3 = \frac{\tau_\eta + \tau_\theta}{\tau_\eta + \tau_\theta + \tau_\beta + \sigma^2}$$

Level 4:

$$\rho_4 = \frac{\tau_\eta}{\tau_\eta + \tau_\theta + \tau_\beta + \sigma^2}$$

Note the level-1 intraclass correlation is undefined and that by definition $\rho_2 \geq \rho_3 \geq \rho_4$. In the case of two-level models the intraclass correlation coefficient is the same as the proportion of the variance in the outcome that is between groups, specifically $\rho = \tau_\beta / (\tau_\beta + \sigma^2)$. More generally, the intraclass correlation coefficient

of the highest level is equivalent to the proportion of the variance at that level. As such these two variance measurements are often confused and incorrectly specified.

The second order specification for the 4-level hierarchical model permits random intercepts, which account for the unique effects of each household, cluster, and state on the anthropometric indicator outcome variable.

Child-level:

$$Y_{ijkl} = \beta_{0jkl} + \sum_{p=1}^P \beta_{pjkl} X_{.1pijkl} + e_{ijkl}$$

Y_{ijkl} : anthropometric indicator of child i , in household j , in cluster k , in state l .

β_{0jkl} : child-level intercept for household j , in cluster k , in state l , which varies between children according to the household-level specification.

β_{pjkl} : child-level fixed effects coefficients for each child-level characteristic $X_{.1pijkl}$.

$X_{.1pijkl}$: $p = 1, \dots, P$ child-level characteristics.

e_{ijkl} : random child effect, deviation of child $ijkl$'s indicator from the predicted indicator; $\sim N(0, \sigma^2)$.

Household-level:

$$\beta_{0jkl} = \theta_{00kl} + \sum_{q=1}^Q \theta_{0qkl} X_{.2qjkl} + r_{0jkl}$$

$$\beta_{pjkl} = \theta_{p0kl} \forall p$$

θ_{00kl} : household-level intercept for cluster k , in state l , which varies between households according to the cluster-level specification.

θ_{0qkl} : household-level fixed effects coefficients for each household-level characteristic $X_{.2qjkl}$.

θ_{p0kl} : equivalent child-level fixed effects in household-level notation.

$X.2_{qjkl}$: $q = 1, \dots, Q$ household-level characteristics.

r_{0jkl} : random household effect, deviation of household jkl 's indicator from the predicted indicator; $\sim N(0, \tau_\beta)$.

Cluster-level:

$$\theta_{00kl} = \eta_{000l} + \sum_{r=1}^R \eta_{00rl} X.3_{rkl} + v_{00kl}$$

$$\theta_{0qkl} = \eta_{0q0l} \forall q$$

$$\theta_{p0kl} = \eta_{p00l} \forall p$$

η_{000l} : cluster-level intercept for state l , which varies between clusters according to the state-level specification.

η_{00rl} : cluster-level fixed effects coefficients for each cluster-level characteristic $X.3_{rkl}$.

η_{0q0l} : equivalent household-level fixed effects in cluster-level notation.

η_{p00l} : equivalent child-level fixed effects in cluster-level notation.

$X.3_{rkl}$: $r = 1, \dots, R$ cluster-level characteristics.

v_{00kl} : random cluster effect, deviation of cluster kl 's indicator from the predicted indicator; $\sim N(0, \tau_\theta)$.

State-level:

$$\eta_{000l} = \gamma_{0000} + \sum_{s=1}^S \gamma_{000s} X.4_{sl} + u_{000l}$$

$$\eta_{00rl} = \gamma_{00r0} \forall r$$

$$\eta_{0q0l} = \gamma_{0q00} \forall q$$

$$\eta_{p00l} = \gamma_{p000} \forall p$$

γ_{0000} : grand intercept.

γ_{000s} : state-level fixed effects coefficients for each state-level characteristic $X.4_{sl}$.

γ_{00r0} : equivalent cluster-level fixed effects in state-level notation.

γ_{0q00} : equivalent household-level fixed effects in state-level notation.

γ_{p000} : equivalent child-level fixed effects in state-level notation.

$X.4_{sl}$: $s = 1, \dots, S$ state-level characteristics.

u_{000l} : random state effect, deviation of state l 's indicator from the predicted indicator; $\sim N(0, \tau_\eta)$.

The mixed model is given by,

$$Y_{ijkl} = \gamma_{0000} + \sum_{p=1}^P \gamma_{p000} X.1_{pijkl} + \sum_{q=1}^Q \gamma_{0q00} X.2_{qjkl} + \sum_{r=1}^R \gamma_{00r0} X.3_{rkl} \\ + \sum_{s=1}^S \gamma_{000s} X.4_{sl} + u_{000l} + v_{00kl} + r_{0jkl} + e_{ijkl}$$

where it is easier to parse the model composition in terms of γ representing the fixed effects and u_{000l} , v_{00kl} , r_{0jkl} , and e_{ijkl} representing the random effects.

Although there exist many alternative permutations and liminal model sub-specifications, the most general specification for the 4-level hierarchical model permits random intercepts and random slopes for each of the intercepts and coefficients for each of the four levels.

Child-level:

$$Y_{ijkl} = \sum_{p=0}^P \beta_{pjkl} X.1_{pijkl} + e_{ijkl}$$

Y_{ijkl} : anthropometric indicator of child i , in household j , in cluster k , in state l .

β_{pjkl} : child-level coefficients for each child-level characteristic $X.1_{pijkl}$.

$X.1_{pijkl}$: $p = 0, \dots, P$ child-level characteristics, assuming $X.1_{0ijkl} = 1 \forall i$ to specify the intercept for household j , in cluster k , in state l .

e_{ijkl} : random child effect, deviation of child $ijkl$'s indicator from the predicted indicator; $\sim N(0, \sigma^2)$.

Household-level:

$$\beta_{pjkl} = \sum_{q=0}^{Q_p} \theta_{pqkl} X.2_{qjkl} + r_{pjkl} \forall p$$

θ_{pqkl} : household-level coefficients for each household-level characteristic $X.2_{qjkl}$.

$X.2_{qjkl}$: $q = 0, \dots, Q_p$ household-level characteristics, assuming $X.2_{0jkl} = 1 \forall j$ to specify the intercept for cluster k , in state l . Each $\beta_p \forall p$ may have a unique set of household-level characteristics, $X.2_{qjkl}$, $q = 0, \dots, Q_p$.

r_{pjkl} : random household effect, deviation of household jkl 's indicator from the predicted indicator; $\sim N(0, \tau_\beta)$.

Cluster-level:

$$\theta_{pqkl} = \sum_{r=0}^{R_{pq}} \eta_{pqrkl} X.3_{rkl} + v_{pqkl} \forall p, q$$

η_{pqrkl} : cluster-level coefficients for each cluster-level characteristic $X.3_{rkl}$.

$X.3_{rkl}$: $r = 0, \dots, R_{pq}$ cluster-level characteristics, assuming $X.3_{0kl} = 1 \forall k$ to specify the intercept for state l . Each $\theta_{pq} \forall p, q$ may have a unique set of cluster-level characteristics, $X.3_{rkl}$, $r = 0, \dots, R_{pq}$.

v_{pqkl} : random cluster effect, deviation of cluster kl 's indicator from the predicted indicator; $\sim N(0, \tau_\theta)$.

State-level:

$$\eta_{pqrl} = \sum_{s=0}^{S_{rpq}} \gamma_{pqrs} X.4_{sl} + u_{pqrl} \quad \forall p, q, r$$

γ_{pqrs} : state-level coefficients for each state-level characteristic $X.4_{sl}$.

$X.4_{sl}$: $s = 0, \dots, S_{rpq}$ state-level characteristics, assuming $X.4_{0l} = 1 \quad \forall l$ to specify the intercept for the state level model. Each $\gamma_{pqr} \quad \forall p, q, r$ may have a unique set of state-level characteristics, $X.4_{sl}, s = 0, \dots, S_{rpq}$.

u_{pqrl} : random state effect, deviation of state l 's indicator from the predicted indicator; $\sim N(0, \tau_\eta)$.

The mixed model is given by,

$$\begin{aligned} Y_{ijkl} = & \sum_{p=0}^P X.1_{pijkl} \sum_{q=0}^{Q_p} X.2_{qjkl} \sum_{r=0}^{R_{pq}} X.3_{rkl} \sum_{s=0}^{S_{rpq}} \gamma_{pqrs} X.4_{sl} \\ & + \sum_{p=0}^P X.1_{pijkl} \sum_{q=0}^{Q_p} X.2_{qjkl} \sum_{r=0}^{R_{pq}} u_{pqrl} X.3_{rkl} \\ & + \sum_{p=0}^P X.1_{pijkl} \sum_{q=0}^{Q_p} v_{pqkl} X.2_{qjkl} \\ & + \sum_{p=0}^P r_{pjkl} X.1_{pijkl} \\ & + e_{ijkl}. \end{aligned}$$

7.3.5.7 Hierarchical Misclassification

The problem of misclassification arises when the perceived outcome Y_i does not correspond to the true outcome Y_i^* (Hausman, 2001; Hausman et al., 1998; Magder & Hughes, 1997). That is, for any number of reasons, one mistakenly thinks a child is healthy when in fact they are malnourished, or vice versa. Unlike measurement error in the classic linear regression model, which only reduces the efficiency of parameter estimates, misclassification errors lead to inconsistent and inefficient parameter estimates in discrete choice models (Hausman et al., 1998; Neuhaus, 1999).

Suppose the observed outcome is a function of the true outcome and misclassification error given by $Y_i = f(Y_i^*, \mu_i)$ and therefore, $\Pr(Y_i^* = 1) \neq \Pr(Y_i = 1)$. For clarity and precision, let \mathcal{M} denote a malnourished outcome \mathcal{H} denote a healthy outcome and $Y_{i\mathcal{M}} = 1$ or the inverse $Y_{i\mathcal{H}} = 0$ means child i is malnourished. By the *law of total probability*, one can decompose the observed probability into constituent conditional probabilities, given by

$$\begin{aligned} \Pr(Y_{i\mathcal{M}} = 1) &= \Pr(Y_{i\mathcal{M}} = 1 | Y_{i\mathcal{M}}^* = 1) \Pr(Y_{i\mathcal{M}}^* = 1) \\ &\quad + \Pr(Y_{i\mathcal{M}} = 1 | Y_{i\mathcal{M}}^* = 0) \Pr(Y_{i\mathcal{M}}^* = 0). \end{aligned}$$

Rearranging terms gives,

$$\begin{aligned} \Pr(Y_{i\mathcal{M}} = 1) &= \Pr(Y_{i\mathcal{M}} = 1 | Y_{i\mathcal{M}}^* = 1) \Pr(Y_{i\mathcal{M}}^* = 1) \\ &\quad + (1 - \Pr(Y_{i\mathcal{H}} = 1 | Y_{i\mathcal{H}}^* = 1)) \Pr(Y_{i\mathcal{H}}^* = 1). \end{aligned}$$

From its decomposed form, the one can define the conditional probabilities as accuracies, given by

$$\alpha_{\mathcal{M}}^{\mathcal{M}} = \Pr(Y_{i\mathcal{M}} = 1 | Y_{i\mathcal{M}}^* = 1),$$

$$\alpha_{\mathcal{H}}^{\mathcal{H}} = \Pr(Y_{i\mathcal{H}} = 1 | Y_{i\mathcal{H}}^* = 1),$$

where the superscript represents conditional probability space. More specifically the conditional probability represented by $\alpha_{\mathcal{M}}^{\mathcal{M}}$ is the *producer's accuracy* of malnutrition or 1 minus the *omission error* of malnutrition also known as *sensitivity*. The conditional probability represented by $\alpha_{\mathcal{H}}^{\mathcal{H}}$ is the *producer's accuracy* of health or 1 minus the *omission error* of health also known as *specificity*. Because they are derived from probabilities, the accuracies and by extension the misclassification errors are bound between zero and one.

Redefining the constituent conditional probabilities as accuracies makes for a tractable model with an intuitive structure. Returning to the original issue of the perceived outcome Y_i not corresponding to the true outcome Y_i^* one can derive a closed-form equation of the relationship, given by

$$\begin{aligned} \Pr(Y_{i\mathcal{M}} = 1) &= \alpha_{\mathcal{M}}^{\mathcal{M}} \Pr(Y_{i\mathcal{M}}^* = 1) + (1 - \alpha_{\mathcal{H}}^{\mathcal{H}}) \Pr(Y_{i\mathcal{H}}^* = 1) \\ &= \alpha_{\mathcal{M}}^{\mathcal{M}} \Pr(Y_{i\mathcal{M}}^* = 1) + (1 - \alpha_{\mathcal{H}}^{\mathcal{H}}) (1 - \Pr(Y_{i\mathcal{M}}^* = 1)) \\ &= 1 - \alpha_{\mathcal{H}}^{\mathcal{H}} + (\alpha_{\mathcal{M}}^{\mathcal{M}} + \alpha_{\mathcal{H}}^{\mathcal{H}} - 1) \Pr(Y_{i\mathcal{M}}^* = 1). \end{aligned}$$

The harm sustained from modeling the outcome as having misclassification error is negligible. Note that if there is no error (i.e., $\alpha_{\mathcal{H}}^{\mathcal{H}} = \alpha_{\mathcal{M}}^{\mathcal{M}} = 1$) the extra terms drop out and the observed outcome is equivalent to the true outcome. Sandler and Rashford note that for “a typical (naïve) estimation procedure the [outcome] probabilities are estimated along with the latent accuracy term, which results in attenuated marginal effects” (2018, p. 532). Hausman and Scott Morton (1994) suggest using a maximum likelihood estimation approach with exogenous conditional probabilities where the accuracy terms are just directly estimable parameters.

Otherwise, the misclassification specification enters the log likelihood function just as the standard specification does. In practical terms, correcting for misclassification errors tends to produce larger standard errors (i.e., less precision) but reveals much larger coefficient estimates (i.e., more oomph).

7.3.6 Results Interpretation

The fully unconditional model only has an intercept term for the fixed effects. The intercept is the estimated log-odds of a child being wasted or stunted. It is more easily interpreted after transforming into a probability through the logistic function i.e., $e^x/(1 + e^x)$. The probability reflects the estimated proportion of children in the sample that are wasted or stunted. Note that the estimated proportions are all considerably less than the observed wasting and stunting proportions. The divide occurs because of the nonlinear relationship between the outcome log-odds, and the outcome probability. The Random effects components reflect the estimated variance partitioning. They are the variance between regions, the variance between states within regions, the variance between clusters within states, and the variance between households between clusters.

Keep in mind the linear probability model assumes constant marginal effects, while the logit and hierarchical model specifications imply diminishing magnitudes of the partial effects. Direct comparisons are therefore dubious and extrapolations beyond a marginal (small) change are inappropriate. The discrete and categorical determinants follow a very precise and intuitive interpretation of their effect. The average marginal effect approach relies on counterfactual reasoning to motivate the conclusions. In effect, there are two hypothetical populations—one of all rural

children, one of all urban children—with the exact same values on the other independent variables in the model. Since the only difference between these two populations is their residence, residence must be the cause of the differences in their likelihood of malnutrition. The continuous determinants also follow a very precise but less intuitive interpretation of their effect. The continuous determinants are given by average adjusted predictions, or the approximate amount of change in the probability of malnutrition produced by a marginal change in any given determinant (e.g., temperature).

Marginal effects provide a good approximation of the amount of change in malnutrition prevalence that will be produced by a 1-unit (or 1-standard deviation) in a determinant. Discrete determinants offer the advantage of only having a single counterfactual and, therefore, a single value of the effect, unlike continuous determinants, which have a theoretically unlimited number of counterfactuals. Instead, the single value given for the continuous determinants is the slope evaluated at the average: a true (linear) marginal effect, if only for some small portion of a greater nonlinear function.

7.3.7 Primary Regression Tables

7.3.7.1 Full Model Results

Table 9. Hierarchical Results: Wasted - Base

Hierarchical Random Intercept	Wasted			
	Nigeria		Kenya	
Average Marginal Effects with 95% Confidence Interval in Brackets				
Sex - Female	-0.012***	[-0.019, -0.0049]	-0.0075***	[-0.011, -0.0036]
Delivery - Clinic	-0.0091**	[-0.017, -0.0011]	-0.010***	[-0.016, -0.0046]
Birth - Singleton	-0.041***	[-0.067, -0.014]	-0.032***	[-0.055, -0.010]
Weaned - By 1 Year Old	-0.0044	[-0.012, 0.0034]	-0.0011	[-0.0048, 0.0026]
Vaccines - Minimum	-0.010**	[-0.020, -0.00032]	-0.0044	[-0.014, 0.0052]
Vaccines - Maximum	-0.010*	[-0.020, 0.000018]	-0.0027	[-0.0085, 0.0032]
Diet - Diverse	0.0077*	[-0.00096, 0.016]	-0.0032	[-0.0090, 0.0025]
Sick - Asymptomatic	-0.010***	[-0.018, -0.0025]	-0.0016	[-0.0058, 0.0026]
Latrine - Improved	-0.0031	[-0.010, 0.0038]	0.0045	[-0.0038, 0.013]
Water - Improved	-0.0026	[-0.012, 0.0066]	-0.00021	[-0.0041, 0.0037]
Residence - Rural	-0.0086	[-0.022, 0.0047]	-0.00029	[-0.0051, 0.0046]
Mothers Education				
Primary	-0.0096**	[-0.018, -0.0012]	-0.011***	[-0.017, -0.0056]
Secondary	-0.020***	[-0.028, -0.011]	-0.0089**	[-0.016, -0.0018]
Higher	-0.040***	[-0.054, -0.027]	-0.017***	[-0.026, -0.0089]
Wealth Index				
Poorer	-0.00059	[-0.0092, 0.0080]	-0.0092***	[-0.016, -0.0022]
Middle	-0.013***	[-0.022, -0.0045]	-0.0079**	[-0.015, -0.00044]
Richer	-0.016***	[-0.028, -0.0042]	-0.011***	[-0.018, -0.0030]
Richest	-0.0095	[-0.025, 0.0063]	-0.012**	[-0.023, -0.0017]
Child's Age	-0.022***	[-0.028, -0.015]	-0.0013	[-0.0038, 0.0012]
Mother's Age	0.0026	[-0.0064, 0.012]	-0.0022	[-0.0065, 0.0020]
Birth Tally	-0.0017	[-0.0039, 0.00054]	0.00069	[-0.00072, 0.0021]
Fixed Effect - Month & Phase	Yes		Yes	
Number of Observations	44,717		26,130	
Log Pseudo Likelihood	-17,439.97		-5,572.63	
Predicted Outcome Analysis				
	Standard	Max Net Benefit	Standard	Max Net Benefit
McIntosh-Dorfman Criterion	1.17	1.71	1.02	1.73
Percent Correctly Classified	86.88	85.76	93.78	79.93
Sensitivity	17.92	85.48	1.52	94.36
Specificity	99.55	85.81	100.00	78.96
Net Benefit	0.027	0.111	0.001	0.046
Cut Off Value	0.5	0.158	0.5	0.045

* p < 0.10, ** p < 0.05, *** p < 0.01

Table 10. Hierarchical Results: Stunted - Base

Hierarchical Random Intercept	Stunted			
	Nigeria		Kenya	
Average Marginal Effects with 95% Confidence Interval in Brackets				
Sex - Female	-0.051***	[-0.059, -0.042]	-0.077***	[-0.091, -0.063]
Delivery - Clinic	-0.022***	[-0.033, -0.011]	-0.046***	[-0.064, -0.028]
Birth - Singleton	-0.13***	[-0.17, -0.091]	-0.23***	[-0.28, -0.18]
Weaned - By 1 Year Old	-0.0031	[-0.016, 0.0100]	-0.011	[-0.028, 0.0065]
Vaccines - Minimum	-0.0056	[-0.028, 0.017]	-0.029**	[-0.055, -0.0020]
Vaccines - Maximum	-0.040***	[-0.060, -0.019]	-0.016**	[-0.031, -0.0022]
Diet - Diverse	-0.020**	[-0.036, -0.0037]	-0.0051	[-0.023, 0.013]
Sick - Asymptomatic	-0.034***	[-0.050, -0.018]	-0.013**	[-0.026, -0.00072]
Latrine - Improved	-0.0043	[-0.020, 0.011]	-0.050***	[-0.072, -0.028]
Water - Improved	0.0020	[-0.011, 0.015]	-0.011	[-0.027, 0.0051]
Residence - Rural	0.015**	[0.000097, 0.029]	-0.014	[-0.036, 0.0080]
Mothers Education				
Primary	-0.015**	[-0.030, -0.000055]	0.025	[-0.0050, 0.056]
Secondary	-0.054***	[-0.074, -0.034]	-0.025	[-0.057, 0.0084]
Higher	-0.13***	[-0.16, -0.10]	-0.059**	[-0.10, -0.013]
Wealth Index				
Poorer	-0.029***	[-0.047, -0.011]	-0.043***	[-0.067, -0.019]
Middle	-0.060***	[-0.082, -0.039]	-0.081***	[-0.11, -0.054]
Richer	-0.12***	[-0.15, -0.099]	-0.10***	[-0.13, -0.069]
Richest	-0.16***	[-0.18, -0.13]	-0.16***	[-0.19, -0.12]
Child's Age	-0.0075	[-0.017, 0.0016]	-0.026***	[-0.033, -0.018]
Mother's Age	-0.036***	[-0.047, -0.025]	-0.046***	[-0.061, -0.031]
Birth Tally	0.0037**	[0.00062, 0.0068]	0.011***	[0.0067, 0.016]
Fixed Effect - Month & Phase	Yes		Yes	
Number of Observations	44,717		26,130	
Log Pseudo Likelihood	-26,250.40		-14,400.96	
Predicted Outcome Analysis				
	Standard	Max Net Benefit	Standard	Max Net Benefit
McIntosh-Dorfman Criterion	1.56	1.60	1.43	1.70
Percent Correctly Classified	80.28	79.20	82.49	85.92
Sensitivity	66.34	82.32	44.31	82.56
Specificity	89.24	77.20	98.29	87.31
Net Benefit	0.217	0.233	0.125	0.205
Cut Off Value	0.5	0.383	0.5	0.317

* p < 0.10, ** p < 0.05, *** p < 0.01

Table 11. ICC and Variance Decomposition: Wasted - Base

Hierarchical Random Intercept	Wasted			
	Nigeria		Kenya	
<i>Random Effect - Variance Component with 95% Confidence Interval in Brackets</i>				
States	0.3	[0.18, 0.41]	0.37	[0.18, 0.57]
Clusters	0.47	[0.31, 0.62]	0.14	[0.025, 0.25]
Households	1.17	[0.86, 1.47]	1.19	[0.70, 1.67]
<i>Intraclass Correlation - Coefficients with 95% Confidence Interval in Brackets</i>				
States	0.057	[0.039, 0.081]	0.075	[0.044, 0.124]
Clusters	0.146	[0.114, 0.186]	0.103	[0.07, 0.149]
Households	0.370	[0.318, 0.425]	0.340	[0.277, 0.41]
<i>Variance Decomposition - Percent by Level</i>				
States	5.66%		7.50%	
Clusters	8.96%		2.77%	
Households	22.34%		23.76%	
Children	63.04%		65.96%	

Table 12. ICC and Variance Decomposition: Stunted - Base

Hierarchical Random Intercept	Stunted			
	Nigeria		Kenya	
Random Effect - Variance Component with 95% Confidence Interval in Brackets				
States	0.26	[0.16, 0.35]	0.099	[0.048, 0.15]
Clusters	0.22	[0.17, 0.26]	0.13	[0.071, 0.20]
Households	0.81	[0.69, 0.93]	1.16	[0.89, 1.43]
Intraclass Correlation - Coefficients with 95% Confidence Interval in Brackets				
States	0.056	[0.039, 0.08]	0.021	[0.013, 0.035]
Clusters	0.103	[0.083, 0.127]	0.050	[0.037, 0.068]
Households	0.281	[0.257, 0.306]	0.297	[0.257, 0.341]
Variance Decomposition - Percent by Level				
States	5.58%		2.12%	
Clusters	4.74%		2.87%	
Households	17.74%		24.71%	
Children	5.58%		2.12%	

Table 13. Hierarchical Results: Wasted - NDVI

Hierarchical Random Intercept	Wasted			
	Nigeria		Kenya	
Average Marginal Effects with 95% Confidence Interval in Brackets				
Sex - Female	-0.013***	[-0.020, -0.0053]	-0.0077***	[-0.012, -0.0037]
Delivery - Clinic	-0.0094**	[-0.018, -0.0010]	-0.010***	[-0.016, -0.0051]
Birth - Singleton	-0.043***	[-0.070, -0.015]	-0.033***	[-0.056, -0.011]
Weaned - By 1 Year Old	-0.0046	[-0.013, 0.0035]	-0.0011	[-0.0050, 0.0027]
Vaccines - Minimum	-0.011**	[-0.021, -0.00032]	-0.0049	[-0.014, 0.0046]
Vaccines - Maximum	-0.010*	[-0.021, 0.00022]	-0.0026	[-0.0086, 0.0034]
Diet - Diverse	0.0080*	[-0.00096, 0.017]	-0.0032	[-0.0091, 0.0028]
Sick - Asymptomatic	-0.010***	[-0.018, -0.0025]	-0.0016	[-0.0060, 0.0028]
Latrine - Improved	-0.0031	[-0.010, 0.0041]	0.0053	[-0.0034, 0.014]
Water - Improved	-0.0034	[-0.013, 0.0063]	-0.00061	[-0.0045, 0.0033]
Residence - Rural	-0.0059	[-0.019, 0.0074]	0.00012	[-0.0049, 0.0051]
Mothers Education				
Primary	-0.0094**	[-0.018, -0.00091]	-0.010***	[-0.016, -0.0042]
Secondary	-0.020***	[-0.029, -0.011]	-0.0079**	[-0.015, -0.00032]
Higher	-0.042***	[-0.055, -0.028]	-0.017***	[-0.026, -0.0080]
Wealth Index				
Poorer	-0.00028	[-0.0092, 0.0087]	-0.0090**	[-0.016, -0.0020]
Middle	-0.013***	[-0.022, -0.0044]	-0.0078**	[-0.015, -0.00036]
Richer	-0.017***	[-0.029, -0.0045]	-0.010***	[-0.018, -0.0029]
Richest	-0.012	[-0.028, 0.0043]	-0.013**	[-0.023, -0.0021]
Child's Age	-0.023***	[-0.029, -0.016]	-0.0013	[-0.0039, 0.0013]
Mother's Age	0.0026	[-0.0067, 0.012]	-0.0024	[-0.0069, 0.0022]
Birth Tally	-0.0017	[-0.0040, 0.00059]	0.00074	[-0.00074, 0.0022]
NDVI	-0.092***	[-0.14, -0.049]	-0.039***	[-0.066, -0.013]
NDVI Anomaly	0.044	[-0.14, 0.23]	0.055	[-0.021, 0.13]
Fixed Effect - Month & Phase	Yes		Yes	
Number of Observations	44,717		26,130	
Log Pseudo Likelihood	-17,433.37		-5,565.46	
Predicted Outcome Analysis				
	Standard	Max Net Benefit	Standard	Max Net Benefit
McIntosh-Dorfman Criterion	1.18	1.71	1.02	1.74
Percent Correctly Classified	86.88	83.38	93.79	80.54
Sensitivity	17.96	88.78	1.70	94.06
Specificity	99.55	82.38	100.00	79.63
Net Benefit	0.027	0.110	0.001	0.047
Cut Off Value	0.5	0.141	0.5	0.046

* p < 0.10, ** p < 0.05, *** p < 0.01

Table 14. Hierarchical Results: Stunted - NDVI

Hierarchical Random Intercept	Stunted			
	Nigeria		Kenya	
Average Marginal Effects with 95% Confidence Interval in Brackets				
Sex - Female	-0.051***	[-0.059, -0.042]	-0.077***	[-0.091, -0.063]
Delivery - Clinic	-0.022***	[-0.033, -0.011]	-0.047***	[-0.065, -0.029]
Birth - Singleton	-0.13***	[-0.17, -0.091]	-0.23***	[-0.28, -0.18]
Weaned - By 1 Year Old	-0.0032	[-0.016, 0.0098]	-0.011	[-0.028, 0.0061]
Vaccines - Minimum	-0.0058	[-0.028, 0.016]	-0.028**	[-0.055, -0.0017]
Vaccines - Maximum	-0.039***	[-0.060, -0.019]	-0.017**	[-0.031, -0.0026]
Diet - Diverse	-0.020**	[-0.037, -0.0038]	-0.0055	[-0.024, 0.013]
Sick - Asymptomatic	-0.034***	[-0.050, -0.018]	-0.013**	[-0.026, -0.00035]
Latrine - Improved	-0.0045	[-0.020, 0.011]	-0.050***	[-0.072, -0.029]
Water - Improved	0.0012	[-0.012, 0.015]	-0.011	[-0.027, 0.0053]
Residence - Rural	0.016**	[0.0029, 0.029]	-0.016	[-0.038, 0.0057]
Mothers Education				
Primary	-0.015*	[-0.030, 0.00016]	0.020	[-0.0090, 0.049]
Secondary	-0.054***	[-0.074, -0.033]	-0.030*	[-0.061, 0.00064]
Higher	-0.13***	[-0.16, -0.10]	-0.064***	[-0.11, -0.020]
Wealth Index				
Poorer	-0.029***	[-0.047, -0.011]	-0.045***	[-0.069, -0.021]
Middle	-0.060***	[-0.082, -0.039]	-0.083***	[-0.11, -0.056]
Richer	-0.12***	[-0.15, -0.099]	-0.10***	[-0.13, -0.070]
Richest	-0.16***	[-0.19, -0.13]	-0.16***	[-0.19, -0.12]
Child's Age	-0.0075	[-0.017, 0.0016]	-0.026***	[-0.033, -0.018]
Mother's Age	-0.036***	[-0.046, -0.025]	-0.046***	[-0.061, -0.031]
Birth Tally	0.0037**	[0.00065, 0.0068]	0.011***	[0.0065, 0.016]
NDVI	-0.066	[-0.19, 0.061]	0.12***	[0.057, 0.18]
NDVI Anomaly	0.30	[-0.20, 0.80]	-0.13	[-0.36, 0.098]
Fixed Effect - Month & Phase	Yes		Yes	
Number of Observations	44,717		26,130	
Log Pseudo Likelihood	-26,247.83		-14,395.99	
Predicted Outcome Analysis				
McIntosh-Dorfman Criterion	Standard	Max Net Benefit	Standard	Max Net Benefit
Percent Correctly Classified	1.56	1.59	1.43	1.70
Sensitivity	80.33	79.39	82.46	85.10
Specificity	66.42	81.37	44.29	84.55
Net Benefit	89.27	78.12	98.26	85.33
Cut Off Value	0.218	0.233	0.125	0.205
	0.5	0.391	0.5	0.304

* p < 0.10, ** p < 0.05, *** p < 0.01

Table 15. ICC and Variance Decomposition: Wasted - NDVI

Hierarchical Random Intercept	Wasted			
	Nigeria		Kenya	
Random Effect - Variance Component with 95% Confidence Interval in Brackets				
States	0.22	[0.13, 0.30]	0.26	[0.10, 0.42]
Clusters	0.47	[0.31, 0.62]	0.15	[0.037, 0.26]
Households	1.17	[0.86, 1.47]	1.19	[0.71, 1.68]
Intraclass Correlation - Coefficients with 95% Confidence Interval in Brackets				
States	0.042	[0.029, 0.061]	0.053	[0.029, 0.096]
Clusters	0.133	[0.104, 0.168]	0.084	[0.059, 0.118]
Households	0.360	[0.308, 0.414]	0.328	[0.263, 0.399]
Variance Decomposition - Percent by Level				
States	4.19%		5.33%	
Clusters	9.09%		3.05%	
Households	22.70%		24.38%	
Children	64.02%		67.24%	

Table 16. ICC and Variance Decomposition: Stunted - NDVI

Hierarchical Random Intercept	Stunted			
	Nigeria		Kenya	
Random Effect - Variance Component with 95% Confidence Interval in Brackets				
States	0.22	[0.11, 0.33]	0.081	[0.038, 0.12]
Clusters	0.21	[0.17, 0.26]	0.13	[0.070, 0.20]
Households	0.81	[0.69, 0.93]	1.16	[0.89, 1.42]
Intraclass Correlation - Coefficients with 95% Confidence Interval in Brackets				
States	0.049	[0.031, 0.078]	0.017	[0.01, 0.029]
Clusters	0.097	[0.074, 0.125]	0.046	[0.033, 0.064]
Households	0.275	[0.25, 0.303]	0.294	[0.253, 0.339]
Variance Decomposition - Percent by Level				
States	4.92%		1.74%	
Clusters	4.73%		2.86%	
Households	17.88%		24.79%	
Children	72.47%		70.61%	

Table 17. Hierarchical Results: Wasted - Precipitation

Hierarchical Random Intercept	Wasted			
	Nigeria		Kenya	
Average Marginal Effects with 95% Confidence Interval in Brackets				
Sex - Female	-0.012***	[-0.019, -0.0051]	-0.0079***	[-0.012, -0.0035]
Delivery - Clinic	-0.0092**	[-0.017, -0.0013]	-0.011***	[-0.016, -0.0053]
Birth - Singleton	-0.042***	[-0.068, -0.015]	-0.034***	[-0.058, -0.011]
Weaned - By 1 Year Old	-0.0044	[-0.012, 0.0035]	-0.0011	[-0.0051, 0.0028]
Vaccines - Minimum	-0.010**	[-0.020, -0.00043]	-0.0044	[-0.014, 0.0054]
Vaccines - Maximum	-0.010*	[-0.021, 0.00016]	-0.0027	[-0.0088, 0.0033]
Diet - Diverse	0.0080*	[0.00098, 0.017]	-0.0034	[-0.0094, 0.0026]
Sick - Asymptomatic	-0.010**	[-0.018, -0.0025]	-0.0017	[-0.0061, 0.0027]
Latrine - Improved	-0.0031	[-0.010, 0.0042]	0.0053	[-0.0035, 0.014]
Water - Improved	-0.0029	[-0.013, 0.0068]	-0.00036	[-0.0044, 0.0037]
Residence - Rural	-0.0080	[-0.022, 0.0061]	-0.000098	[-0.0051, 0.0049]
Mothers Education				
Primary	-0.0094**	[-0.018, -0.00099]	-0.0097***	[-0.016, -0.0037]
Secondary	-0.020***	[-0.029, -0.011]	-0.0069*	[-0.015, 0.00080]
Higher	-0.041***	[-0.054, -0.027]	-0.016***	[-0.025, -0.0073]
Wealth Index				
Poorer	-0.00038	[-0.0093, 0.0085]	-0.0089**	[-0.016, -0.0018]
Middle	-0.013***	[-0.022, -0.0044]	-0.0077**	[-0.015, -0.00015]
Richer	-0.016***	[-0.029, -0.0042]	-0.010***	[-0.018, -0.0027]
Richest	-0.0097	[-0.026, 0.0063]	-0.013**	[-0.023, -0.0018]
Child's Age	-0.022***	[-0.029, -0.016]	-0.0013	[-0.0040, 0.0013]
Mother's Age	0.0026	[-0.0066, 0.012]	-0.0024	[-0.0069, 0.0021]
Birth Tally	-0.0017	[-0.0040, 0.00057]	0.00077	[-0.00068, 0.0022]
Precipitation	-0.0096	[-0.023, 0.0041]	-0.015***	[-0.025, -0.0063]
Precipitation Anomaly	-0.0045	[-0.049, 0.040]	0.011	[-0.0088, 0.031]
Fixed Effect - Month & Phase	Yes		Yes	
Number of Observations	44,717		26,130	
Log Pseudo Likelihood	-17,437.95		-5,564.13	
Predicted Outcome Analysis				
	Standard	Max Net Benefit	Standard	Max Net Benefit
McIntosh-Dorfman Criterion	1.18	1.71	1.02	1.73
Percent Correctly Classified	86.89	86.16	93.78	79.30
Sensitivity	17.96	84.90	1.58	94.66
Specificity	99.55	86.39	100.00	78.27
Net Benefit	0.027	0.111	0.001	0.046
Cut Off Value	0.5	0.161	0.5	0.044

* p < 0.10, ** p < 0.05, *** p < 0.01

Table 18. Hierarchical Results: Stunted - Precipitation

Hierarchical Random Intercept	Stunted			
	Nigeria		Kenya	
Average Marginal Effects with 95% Confidence Interval in Brackets				
Sex - Female	-0.051***	[-0.059, -0.042]	-0.077***	[-0.091, -0.063]
Delivery - Clinic	-0.023***	[-0.034, -0.012]	-0.046***	[-0.064, -0.028]
Birth - Singleton	-0.13***	[-0.17, -0.091]	-0.23***	[-0.28, -0.17]
Weaned - By 1 Year Old	-0.0030	[-0.016, 0.010]	-0.011	[-0.028, 0.0062]
Vaccines - Minimum	-0.0059	[-0.028, 0.016]	-0.029**	[-0.056, -0.0027]
Vaccines - Maximum	-0.040***	[-0.060, -0.019]	-0.017**	[-0.031, -0.0024]
Diet - Diverse	-0.021**	[-0.037, -0.0041]	-0.0053	[-0.024, 0.013]
Sick - Asymptomatic	-0.034***	[-0.050, -0.018]	-0.013**	[-0.026, -0.00028]
Latrine - Improved	-0.0049	[-0.020, 0.010]	-0.050***	[-0.072, -0.028]
Water - Improved	0.00032	[-0.012, 0.013]	-0.011	[-0.027, 0.0047]
Residence - Rural	0.015**	[0.00031, 0.030]	-0.015	[-0.038, 0.0073]
Mothers Education				
Primary	-0.015*	[-0.029, 0.00015]	0.020	[-0.0098, 0.049]
Secondary	-0.053***	[-0.074, -0.033]	-0.031*	[-0.063, 0.00099]
Higher	-0.13***	[-0.16, -0.10]	-0.065***	[-0.11, -0.021]
Wealth Index				
Poorer	-0.028***	[-0.046, -0.0100]	-0.045***	[-0.069, -0.021]
Middle	-0.058***	[-0.080, -0.036]	-0.083***	[-0.11, -0.056]
Richer	-0.12***	[-0.15, -0.096]	-0.10***	[-0.13, -0.070]
Richest	-0.16***	[-0.18, -0.13]	-0.16***	[-0.19, -0.13]
Child's Age	-0.0076	[-0.017, 0.0015]	-0.026***	[-0.033, -0.018]
Mother's Age	-0.036***	[-0.047, -0.025]	-0.046***	[-0.061, -0.031]
Birth Tally	0.0037**	[0.00067, 0.0068]	0.011***	[0.0065, 0.016]
Precipitation	-0.015	[-0.044, 0.014]	0.033**	[0.0033, 0.063]
Precipitation Anomaly	0.052	[-0.010, 0.11]	-0.034	[-0.080, 0.012]
Fixed Effect - Month & Phase	Yes		Yes	
Number of Observations	44,717		26,130	
Log Pseudo Likelihood	-26,245.12		-14,396.91	
Predicted Outcome Analysis				
McIntosh-Dorfman Criterion	Standard	Max Net Benefit	Standard	Max Net Benefit
Percent Correctly Classified	1.56	1.59	1.43	1.70
Sensitivity	80.31	79.32	82.48	85.92
Specificity	66.42	81.63	44.40	82.44
Net Benefit	89.23	77.84	98.24	87.37
Cut Off Value	0.218	0.233	0.125	0.204
	0.5	0.389	0.5	0.318

* p < 0.10, ** p < 0.05, *** p < 0.01

Table 19. ICC and Variance Decomposition: Wasted - Precipitation

Hierarchical Random Intercept	Wasted			
	Nigeria		Kenya	
Random Effect - Variance Component with 95% Confidence Interval in Brackets				
States	0.25	[0.13, 0.38]	0.29	[0.14, 0.44]
Clusters	0.47	[0.31, 0.62]	0.13	[0.016, 0.25]
Households	1.17	[0.86, 1.47]	1.17	[0.68, 1.66]
Intraclass Correlation - Coefficients with 95% Confidence Interval in Brackets				
States	0.049	[0.031, 0.077]	0.060	[0.036, 0.097]
Clusters	0.139	[0.106, 0.181]	0.086	[0.062, 0.119]
Households	0.364	[0.312, 0.42]	0.327	[0.258, 0.404]
Variance Decomposition - Percent by Level				
States	4.89%		5.96%	
Clusters	9.04%		2.68%	
Households	22.52%		24.03%	
Children	63.55%		67.33%	

Table 20. ICC and Variance Decomposition: Stunted - Precipitation

Hierarchical Random Intercept	Stunted			
	Nigeria		Kenya	
<i>Random Effect - Variance Component with 95% Confidence Interval in Brackets</i>				
States	0.22	[0.092, 0.34]	0.099	[0.049, 0.15]
Clusters	0.21	[0.17, 0.26]	0.13	[0.067, 0.20]
Households	0.81	[0.69, 0.93]	1.16	[0.89, 1.43]
<i>Intraclass Correlation - Coefficients with 95% Confidence Interval in Brackets</i>				
States	0.048	[0.027, 0.081]	0.021	[0.013, 0.034]
Clusters	0.095	[0.07, 0.127]	0.049	[0.036, 0.067]
Households	0.274	[0.249, 0.3]	0.297	[0.256, 0.341]
<i>Variance Decomposition - Percent by Level</i>				
States	4.75%		2.11%	
Clusters	4.70%		2.81%	
Households	17.94%		24.74%	
Children	72.61%		70.33%	

Table 21. Hierarchical Results: Wasted - Temperature

Hierarchical Random Intercept	Wasted			
	Nigeria		Kenya	
Average Marginal Effects with 95% Confidence Interval in Brackets				
Sex - Female	-0.013***	[-0.020, -0.0057]	-0.0080***	[-0.012, -0.0038]
Delivery - Clinic	-0.0090**	[-0.018, -0.00020]	-0.011***	[-0.016, -0.0051]
Birth - Singleton	-0.044***	[-0.072, -0.016]	-0.034***	[-0.058, -0.011]
Weaned - By 1 Year Old	-0.0044	[-0.013, 0.0040]	-0.00090	[-0.0049, 0.0031]
Vaccines - Minimum	-0.010*	[-0.021, 0.00041]	-0.0048	[-0.015, 0.0051]
Vaccines - Maximum	-0.010*	[-0.021, 0.00085]	-0.0025	[-0.0086, 0.0037]
Diet - Diverse	0.0084*	[-0.00076, 0.018]	-0.0032	[-0.0093, 0.0030]
Sick - Asymptomatic	-0.011***	[-0.019, -0.0026]	-0.0014	[-0.0058, 0.0030]
Latrine - Improved	-0.0038	[-0.011, 0.0036]	0.0057	[-0.0037, 0.015]
Water - Improved	-0.0038	[-0.014, 0.0061]	-0.00073	[-0.0049, 0.0034]
Residence - Rural	-0.0098	[-0.024, 0.0046]	0.00098	[-0.0044, 0.0064]
Mothers Education				
Primary	-0.0085*	[-0.017, 0.00010]	-0.010***	[-0.016, -0.0040]
Secondary	-0.019***	[-0.028, -0.010]	-0.0072*	[-0.015, 0.00056]
Higher	-0.041***	[-0.055, -0.028]	-0.017***	[-0.026, -0.0076]
Wealth Index				
Poorer	0.0013	[-0.0082, 0.011]	-0.0086**	[-0.016, -0.0014]
Middle	-0.011**	[-0.021, -0.0018]	-0.0070*	[-0.015, 0.00061]
Richer	-0.014**	[-0.027, -0.0012]	-0.0093**	[-0.017, -0.0016]
Richest	-0.0067	[-0.024, 0.010]	-0.011**	[-0.022, -0.00058]
Child's Age	-0.024***	[-0.030, -0.017]	-0.0014	[-0.0041, 0.0013]
Mother's Age	0.0030	[-0.0066, 0.013]	-0.0020	[-0.0066, 0.0026]
Birth Tally	-0.0018	[-0.0042, 0.00061]	0.00066	[-0.00087, 0.0022]
Temperature	0.012***	[0.0079, 0.015]	0.0024***	[0.0012, 0.0036]
Temperature Anomaly	-0.027**	[-0.052, -0.0026]	-0.000052	[-0.0062, 0.0061]
Fixed Effect - Month & Phase	Yes		Yes	
Number of Observations	44,717		26,130	
Log Pseudo Likelihood	-17,419.90		-5,561.32	
Predicted Outcome Analysis				
	Standard	Max Net Benefit	Standard	Max Net Benefit
McIntosh-Dorfman Criterion	1.18	1.71	1.02	1.73
Percent Correctly Classified	86.92	84.76	93.79	79.12
Sensitivity	18.22	87.14	1.70	95.33
Specificity	99.54	84.32	100.00	78.03
Net Benefit	0.028	0.111	0.001	0.046
Cut Off Value	0.5	0.150	0.5	0.043

* p < 0.10, ** p < 0.05, *** p < 0.01

Table 22. Hierarchical Results: Stunted - Temperature

Hierarchical Random Intercept	Stunted			
	Nigeria		Kenya	
Average Marginal Effects with 95% Confidence Interval in Brackets				
Sex - Female	-0.051***	[-0.059, -0.042]	-0.077***	[-0.091, -0.063]
Delivery - Clinic	-0.022***	[-0.034, -0.011]	-0.047***	[-0.065, -0.028]
Birth - Singleton	-0.13***	[-0.17, -0.091]	-0.23***	[-0.28, -0.18]
Weaned - By 1 Year Old	-0.0031	[-0.016, 0.0099]	-0.012	[-0.029, 0.0049]
Vaccines - Minimum	-0.0057	[-0.028, 0.016]	-0.029**	[-0.055, -0.0022]
Vaccines - Maximum	-0.040***	[-0.060, -0.019]	-0.017**	[-0.031, -0.0029]
Diet - Diverse	-0.020**	[-0.037, -0.0038]	-0.0061	[-0.024, 0.012]
Sick - Asymptomatic	-0.034***	[-0.050, -0.018]	-0.014**	[-0.027, -0.0011]
Latrine - Improved	-0.0049	[-0.021, 0.011]	-0.051***	[-0.072, -0.029]
Water - Improved	0.0019	[-0.011, 0.015]	-0.010	[-0.026, 0.0060]
Residence - Rural	0.014*	[-0.000030, 0.029]	-0.018*	[-0.040, 0.0033]
Mothers Education				
Primary	-0.015*	[-0.030, 0.000066]	0.019	[-0.010, 0.048]
Secondary	-0.054***	[-0.074, -0.034]	-0.033**	[-0.064, -0.0012]
Higher	-0.13***	[-0.16, -0.10]	-0.066***	[-0.11, -0.022]
Wealth Index				
Poorer	-0.029***	[-0.046, -0.012]	-0.048***	[-0.072, -0.024]
Middle	-0.060***	[-0.081, -0.038]	-0.087***	[-0.11, -0.060]
Richer	-0.12***	[-0.15, -0.098]	-0.11***	[-0.14, -0.075]
Richest	-0.16***	[-0.18, -0.13]	-0.16***	[-0.20, -0.13]
Child's Age	-0.0076	[-0.017, 0.0016]	-0.026***	[-0.033, -0.018]
Mother's Age	-0.036***	[-0.047, -0.025]	-0.048***	[-0.062, -0.033]
Birth Tally	0.0037**	[0.00065, 0.0068]	0.012***	[0.0071, 0.016]
Temperature	-0.0026	[-0.013, 0.0073]	-0.0092***	[-0.012, -0.0061]
Temperature Anomaly	-0.018	[-0.058, 0.021]	0.010	[-0.0043, 0.024]
Fixed Effect - Month & Phase	Yes		Yes	
Number of Observations	44,717		26,130	
Log Pseudo Likelihood	-26,249.11		-14,384.23	
Predicted Outcome Analysis				
McIntosh-Dorfman Criterion	Standard	Max Net Benefit	Standard	Max Net Benefit
	1.56	1.59	1.43	1.70
Percent Correctly Classified	80.28	79.23	82.58	84.96
Sensitivity	66.35	82.06	44.59	84.48
Specificity	89.24	77.42	98.30	85.16
Net Benefit	0.217	0.233	0.126	0.204
Cut Off Value	0.5	0.385	0.5	0.303

* p < 0.10, ** p < 0.05, *** p < 0.01

Table 23. ICC and Variance Decomposition: Wasted - Temperature

Hierarchical Random Intercept	Wasted			
	Nigeria		Kenya	
<i>Random Effect - Variance Component with 95% Confidence Interval in Brackets</i>				
States	0.16	[0.078, 0.25]	0.33	[0.14, 0.52]
Clusters	0.46	[0.31, 0.62]	0.13	[0.012, 0.25]
Households	1.17	[0.87, 1.47]	1.19	[0.71, 1.68]
<i>Intraclass Correlation - Coefficients with 95% Confidence Interval in Brackets</i>				
States	0.032	[0.02, 0.051]	0.067	[0.038, 0.113]
Clusters	0.123	[0.092, 0.161]	0.093	[0.064, 0.134]
Households	0.352	[0.298, 0.411]	0.334	[0.266, 0.41]
<i>Variance Decomposition - Percent by Level</i>				
States	3.18%		6.66%	
Clusters	9.07%		2.65%	
Households	22.98%		24.11%	
Children	64.77%		66.58%	

Table 24. ICC and Variance Decomposition: Stunted - Temperature

Hierarchical Random Intercept	Stunted			
	Nigeria		Kenya	
<i>Random Effect - Variance Component with 95% Confidence Interval in Brackets</i>				
States	0.27	[0.16, 0.38]	0.07	[0.030, 0.11]
Clusters	0.21	[0.17, 0.26]	0.12	[0.065, 0.18]
Households	0.81	[0.69, 0.93]	1.16	[0.89, 1.43]
<i>Intraclass Correlation - Coefficients with 95% Confidence Interval in Brackets</i>				
States	0.058	[0.039, 0.086]	0.015	[0.009, 0.026]
Clusters	0.105	[0.082, 0.134]	0.042	[0.03, 0.058]
Households	0.282	[0.259, 0.307]	0.291	[0.251, 0.335]
<i>Variance Decomposition - Percent by Level</i>				
States	5.84%		1.50%	
Clusters	4.67%		2.69%	
Households	17.71%		24.95%	
Children	71.78%		70.87%	

7.3.8 Additional Regression Figures

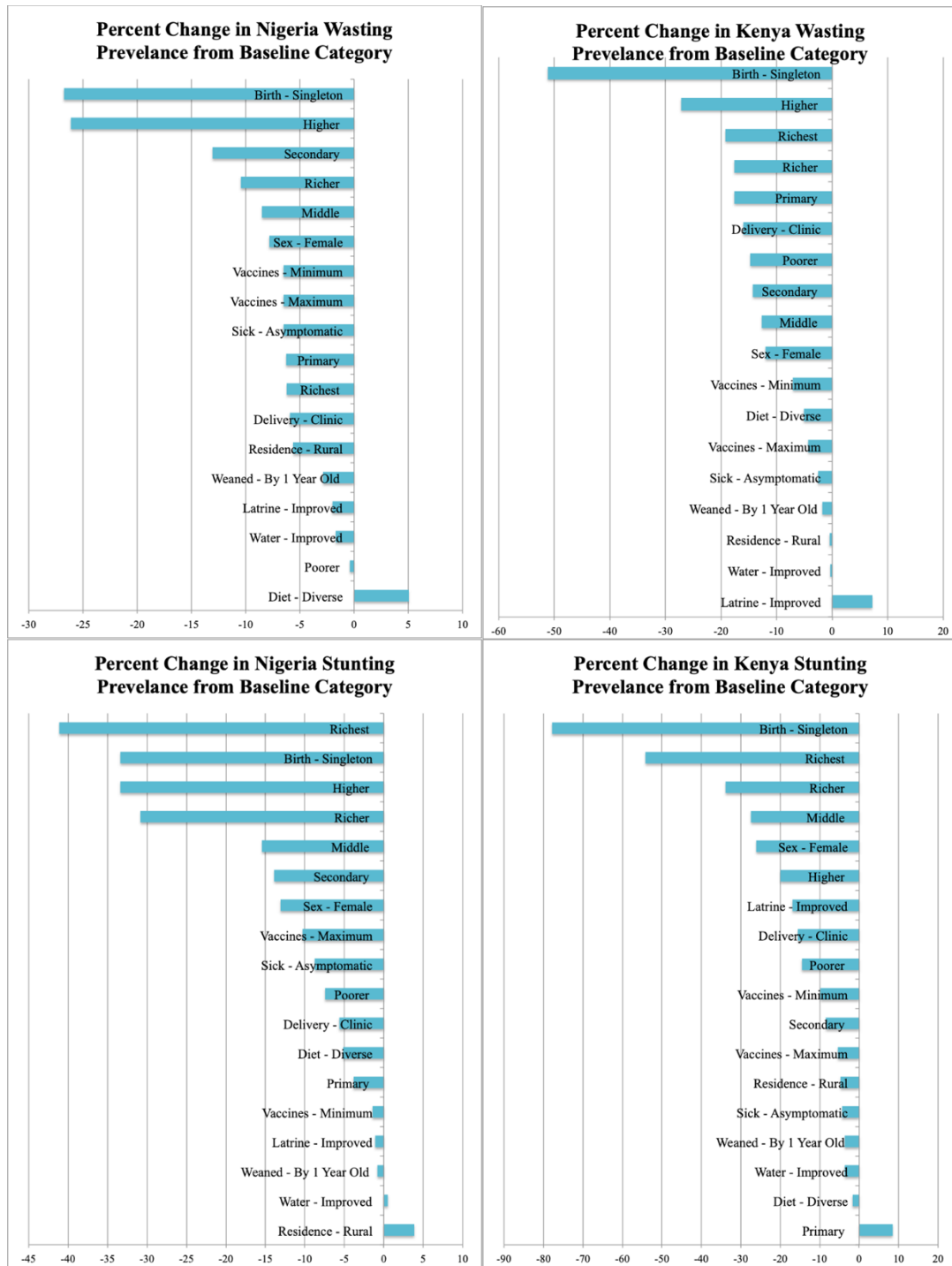


Figure 20: Ranked effect of categorical malnutrition determinants. Derived from Table 7 and Table 8.

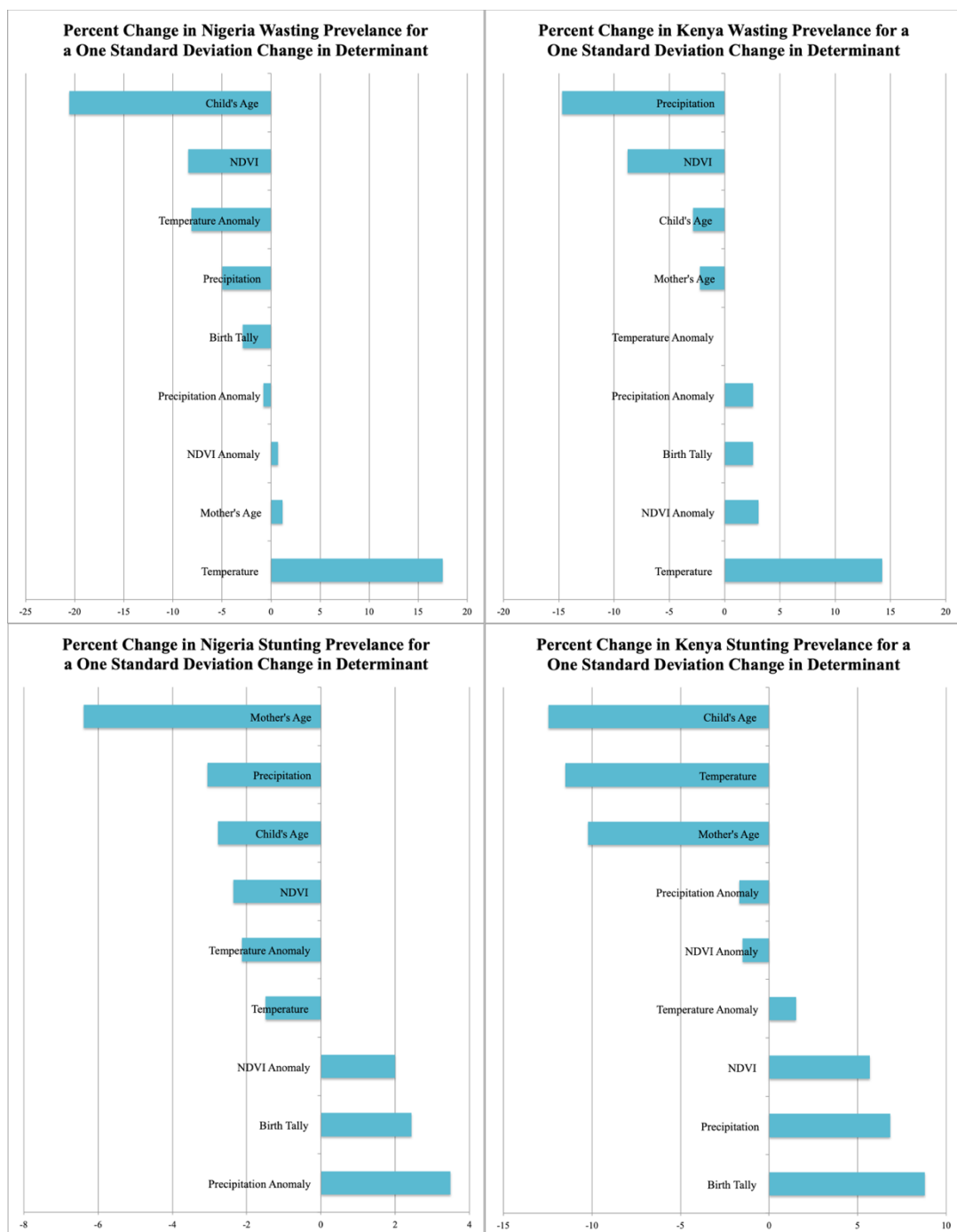


Figure 21: Ranked effect of continuous malnutrition determinants. Derived from Table 7 and Table 8.

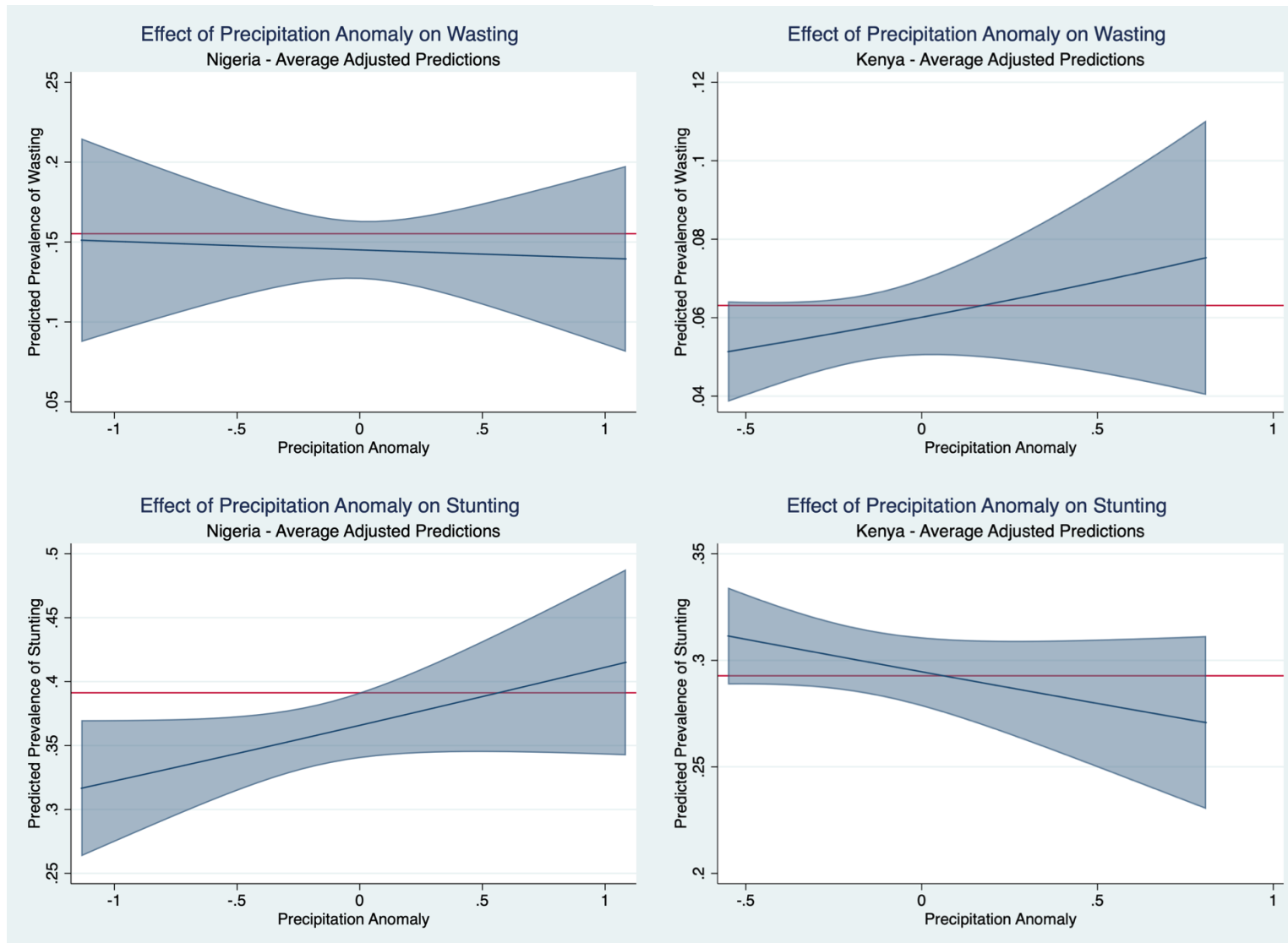


Figure 22: Effect of precipitation anomaly on average predicted probability of malnutrition. The horizontal axis is the in-sample range of average total monthly rainfall anomaly (dm) during the preceding growing season. The horizontal red line demarks the observed malnutrition prevalence and the sloped blue line illustrates how much the expected prevalence rates change as precipitation anomaly changes. The shaded blue corresponds to a 95% confidence interval band on the estimate.

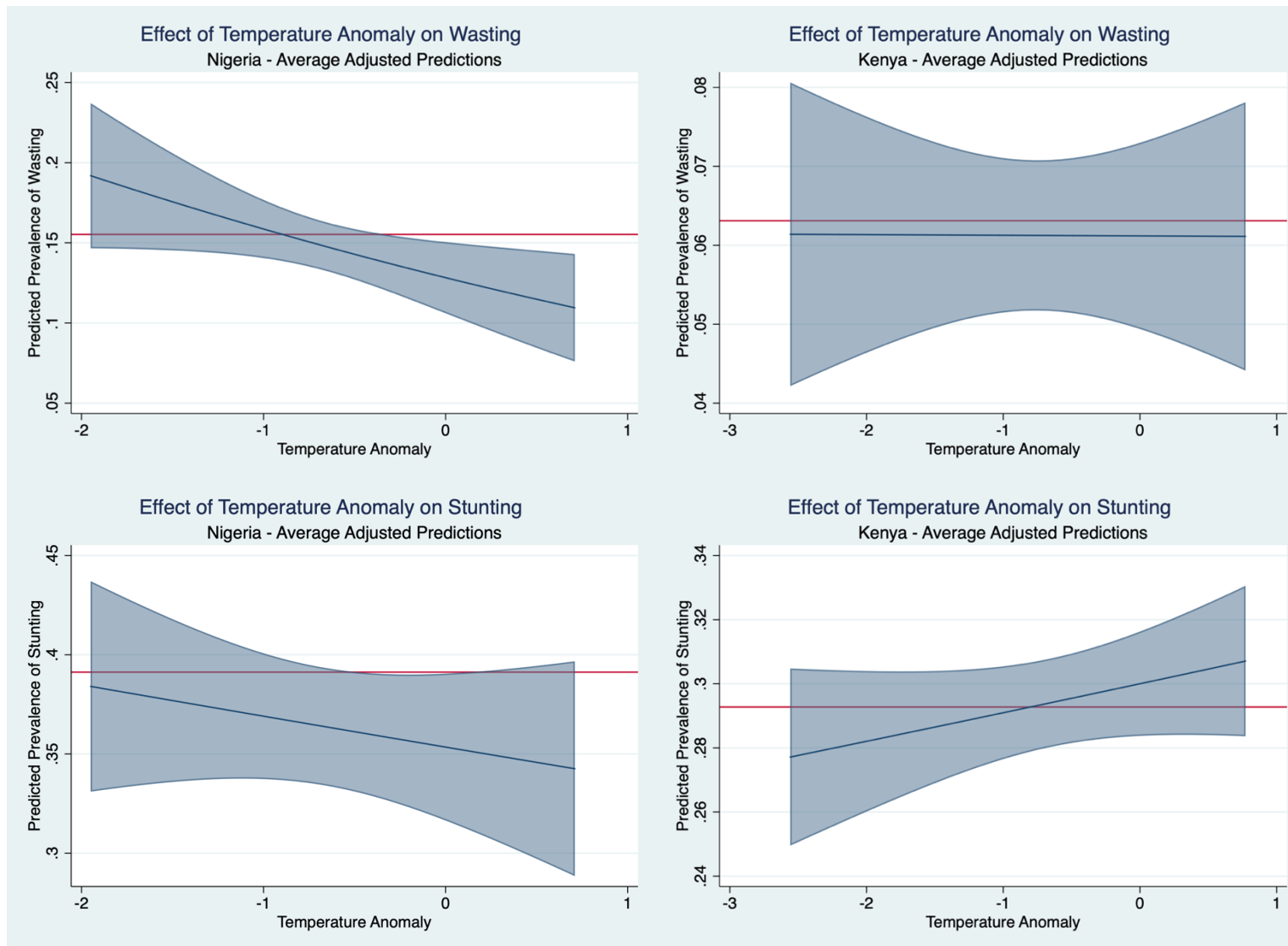


Figure 23: Effect of temperature anomaly on average predicted probability of malnutrition. The horizontal axis is the in-sample range of average maximum monthly temperature anomaly ($^{\circ}\text{C}$) during the preceding growing season. The horizontal red line demarks the observed malnutrition prevalence and the sloped blue line illustrates how much the expected prevalence rates change as temperature anomaly changes. The shaded blue corresponds to a 95% confidence interval band on the estimate.

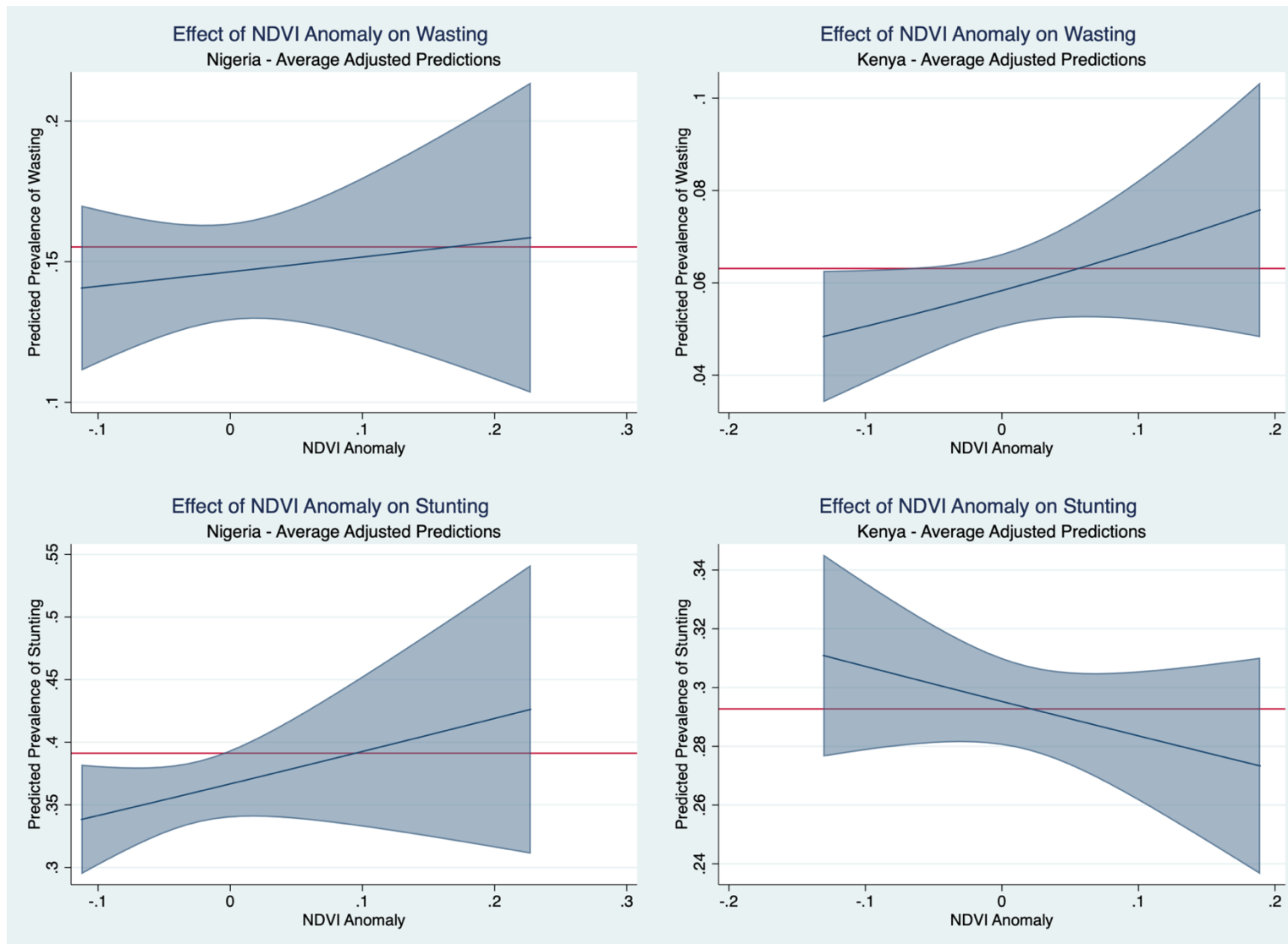


Figure 24: Effect of NDVI anomaly on average predicted probability of malnutrition. The horizontal axis is the in-sample range of the unit-less NDVI anomaly for the three greenest months during the preceding growing season. The horizontal red line demarks the observed malnutrition prevalence and the sloped blue line illustrates how much the expected prevalence rates change as NDVI anomaly changes. The shaded blue corresponds to a 95% confidence interval band on the estimate.

7.3.9 Discrete Results Exegesis

- A child being female reduces their probability of wasting by 1.2 percentage points in Nigeria and 0.75 percentage points in Kenya, and reduces their probability of stunting by 5.1 percentage points in Nigeria and 7.7 percentage points in Kenya.
- Having clinical deliveries reduces the prevalence of wasting by 0.91 percentage points in Nigeria and 1 percentage point in Kenya, and reduces the prevalence of stunting by 2.2 percentage points in Nigeria and 4.6 percentage points in Kenya.
- Children of singleton births reduce the prevalence of wasting by 4.1 percentage points in Nigeria and 3.2 percentage points in Kenya, and reduce the prevalence of stunting by 13 percentage points in Nigeria and 23 percentage points in Kenya.
- Children who are weaned by 1 year old reduce their probability of wasting by 0.44 percentage points in Nigeria and 0.11 percentage points in Kenya, and reduce their probability of stunting by 0.31 percentage points in Nigeria and 1.1 percentage points in Kenya.
- Children who have at least one vaccine reduce their probability of wasting by 1 percentage point in Nigeria and 0.44 percentage points in Kenya, and reduce their probability of stunting by 0.56 percentage points in Nigeria and 2.9 percentage points in Kenya.
- Children who have all their vaccines reduce their probability of wasting by 1 percentage point in Nigeria and 0.27 percentage points in Kenya, and reduce their probability of stunting by 4 percentage points in Nigeria and 1.6 percentage points in Kenya. Surprisingly the benefits to malnutrition are actually smaller in Kenya with more vaccines however the confidence interval overlap of the two measures is enough to make them essentially indistinguishable.
- Children with a diverse diet reduce their probability of wasting by 0.32 percentage points in Kenya, and reduce their probability of stunting by 2 percentage points in Nigeria and 0.51 percentage points in Kenya, whereas a diverse diet increases the probability of wasting by 0.77 percentage points in Nigeria.

- A child being asymptomatic of fever, cough, or diarrhea reduces their probability of wasting by 1 percentage point in Nigeria and 0.16 percentage points in Kenya, and reduces their probability of stunting by 3.4 percentage points in Nigeria and 1.3 percentage points in Kenya.
- Having access to an improved latrine reduces the prevalence of wasting by 0.31 percentage points in Nigeria, and reduces the prevalence of stunting by 0.43 percentage points in Nigeria and 5 percentage points in Kenya, whereas improved latrine access increases the probability of wasting by 0.45 percentage points in Kenya.
- Having access to improved water reduces the prevalence of wasting by 0.26 percentage points in Nigeria and 0.02 percentage points in Kenya, and reduces the prevalence of stunting by 1.1 percentage points in Kenya, whereas improved water access increases the probability of stunting by 0.2 percentage points in Kenya.
- Children living in rural areas have reduced prevalence of wasting by 0.86 percentage points in Nigeria and 0.03 percentage points in Kenya, and reduced prevalence of stunting by 1.4 percentage points in Kenya, whereas rural residence increases the prevalence of stunting by 1.5 percentage points in Nigeria.

7.3.10 Ancillary Regression Tables

7.3.10.1 Unconditional Hierarchical Model Results

Table 25. Hierarchical Results: Wasted - Fully Unconditional

Hierarchical Fully Unconditional	Wasted			
	Nigeria		Kenya	
<i>Fixed Effect - Coefficient with 95% Confidence Interval in Brackets</i>				
Constant	-2.37	[-2.56, -2.17]	-3.53	[-3.85, -3.20]
Observations	48,068		28,241	
Distribution Family	Bernoulli		Bernoulli	
Link Function	Logit		Logit	
<i>Random Effect - Variance Component with 95% Confidence Interval in Brackets</i>				
States	0.35	[0.22, 0.49]	0.6	[0.34, 0.86]
Clusters	0.47	[0.31, 0.64]	0.34	[0.20, 0.47]
Households	0.87	[0.62, 1.13]	1.06	[0.66, 1.46]
<i>Intraclass Correlation - Coefficients with 95% Confidence Interval in Brackets</i>				
States	0.071	[0.051, 0.098]	0.110	[0.074, 0.17]
Clusters	0.170	[0.13, 0.21]	0.180	[0.14, 0.23]
Households	0.340	[0.29, 0.4]	0.380	[0.33, 0.43]
<i>Variance Decomposition - Percent by Level</i>				
States	7.09%		11.35%	
Clusters	9.48%		6.35%	
Households	17.50%		20.09%	
Children	65.93%		62.22%	

Table 26. Hierarchical Results: Stunted - Fully Unconditional

Hierarchical Fully Unconditional	Stunted			
	Nigeria		Kenya	
Fixed Effect - Coefficient with 95% Confidence Interval in Brackets				
Constant	-0.79	[-1.04, - 0.54]	-1.14	[-1.25, -1.03]
Observations	48,068		28,241	
Distribution Family	Bernoulli		Bernoulli	
Link Function	Logit		Logit	
Random Effect - Variance Component with 95% Confidence Interval in Brackets				
States	0.52	[0.36, 0.69]	0.086	[0.040, 0.13]
Clusters	0.33	[0.26, 0.41]	0.28	[0.21, 0.34]
Households	0.64	[0.51, 0.76]	0.92	[0.70, 1.14]
Intraclass Correlation - Coefficients with 95% Confidence Interval in Brackets				
States	0.110	[0.082, 0.14]	0.019	[0.011, 0.031]
Clusters	0.180	[0.15, 0.21]	0.080	[0.066, 0.096]
Households	0.310	[0.28, 0.35]	0.280	[0.25, 0.32]
Variance Decomposition - Percent by Level				
States	10.94%		1.87%	
Clusters	6.99%		6.11%	
Households	13.31%		20.08%	
Children	68.77%		71.94%	

7.3.10.2 Linear Probability Model Results

Table 27. LPM Results: Wasted - Base

Linear Probability Model	Wasted			
	Nigeria		Kenya	
Average Marginal Effects with 95% Confidence Interval in Brackets				
Sex – Female	-0.014***	[-0.021, -0.0077]	-0.013***	[-0.018, -0.0067]
Delivery – Clinic	-0.029***	[-0.037, -0.021]	-0.017***	[-0.023, -0.010]
Birth – Singleton	-0.041***	[-0.062, -0.021]	-0.046***	[-0.069, -0.024]
Weaned – By 1 Year Old	-0.0050	[-0.013, 0.0025]	-0.0048	[-0.013, 0.0035]
Vaccines – Minimum	-0.021***	[-0.030, -0.013]	-0.029***	[-0.048, -0.0087]
Vaccines – Maximum	-0.010**	[-0.019, -0.0014]	-0.0035	[-0.0097, 0.0026]
Diet – Diverse	-0.00040	[-0.0082, 0.0074]	-0.0098**	[-0.018, -0.0014]
Sick – Asymptomatic	-0.016***	[-0.023, -0.0079]	0.0022	[-0.0037, 0.0080]
Latrine – Improved	0.025***	[0.018, 0.033]	0.0070*	[-0.00039, 0.014]
Water – Improved	0.017***	[0.0093, 0.025]	-0.0045	[-0.011, 0.0022]
Residence – Rural	-0.020***	[-0.028, -0.011]	-0.0068*	[-0.014, 0.00062]
Mothers Education				
Primary	-0.048***	[-0.057, -0.039]	-0.073***	[-0.083, -0.063]
Secondary	-0.063***	[-0.073, -0.053]	-0.069***	[-0.081, -0.058]
Higher	-0.088***	[-0.10, -0.072]	-0.077***	[-0.091, -0.063]
Wealth Index				
Poorer	-0.014**	[-0.025, -0.0028]	-0.025***	[-0.034, -0.016]
Middle	-0.037***	[-0.049, -0.026]	-0.024***	[-0.034, -0.014]
Richer	-0.038***	[-0.050, -0.025]	-0.026***	[-0.036, -0.015]
Richest	-0.029***	[-0.045, -0.014]	-0.032***	[-0.044, -0.019]
Child’s Age	-0.024***	[-0.027, -0.020]	-0.0023	[-0.0053, 0.00072]
Mother’s Age	-0.0078**	[-0.015, -0.00033]	-0.00065	[-0.0075, 0.0062]
Birth Tally	0.00035	[-0.0018, 0.0025]	-0.00018	[-0.0024, 0.0020]
Fixed Effect – Month & Phase	Yes		Yes	
Number of Observations	44,735		26,299	
R ²	0.045		0.039	
Outlying Predictions Count	723		227	

* p < 0.10, ** p < 0.05, *** p < 0.01

Table 28. LPM Results: Stunted - Base

Linear Probability Model	Nigeria		Kenya	
<i>Average Marginal Effects with 95% Confidence Interval in Brackets</i>				
Sex – Female	-0.042***	[-0.051, -0.034]	-0.068***	[-0.079, -0.058]
Delivery – Clinic	-0.053***	[-0.064, -0.042]	-0.044***	[-0.057, -0.031]
Birth – Singleton	-0.10***	[-0.13, -0.075]	-0.20***	[-0.23, -0.16]
Weaned – By 1 Year Old	-0.012**	[-0.022, -0.0014]	-0.015*	[-0.031, 0.00037]
Vaccines – Minimum	0.0036	[-0.0073, 0.015]	-0.014	[-0.042, 0.013]
Vaccines – Maximum	-0.055***	[-0.068, -0.043]	-0.013**	[-0.025, -0.00080]
Diet – Diverse	-0.018***	[-0.029, -0.0082]	0.0031	[-0.013, 0.019]
Sick – Asymptomatic	-0.024***	[-0.034, -0.015]	-0.011**	[-0.022, -0.00021]
Latrine – Improved	0.025***	[0.015, 0.034]	-0.039***	[-0.053, -0.025]
Water – Improved	0.0048	[-0.0051, 0.015]	-0.014**	[-0.027, -0.0021]
Residence – Rural	0.0035	[-0.0077, 0.015]	-0.0086	[-0.023, 0.0058]
Mothers Education				
Primary	-0.073***	[-0.085, -0.061]	0.063***	[0.047, 0.079]
Secondary	-0.12***	[-0.13, -0.11]	0.017	[-0.0035, 0.037]
Higher	-0.16***	[-0.18, -0.14]	-0.0019	[-0.028, 0.024]
Wealth Index				
Poorer	-0.035***	[-0.049, -0.022]	-0.038***	[-0.056, -0.021]
Middle	-0.077***	[-0.092, -0.062]	-0.076***	[-0.095, -0.058]
Richer	-0.13***	[-0.14, -0.11]	-0.088***	[-0.11, -0.068]
Richest	-0.16***	[-0.18, -0.14]	-0.13***	[-0.15, -0.11]
Child’s Age	-0.0045*	[-0.0089, 0.000037]	-0.023***	[-0.029, -0.017]
Mother’s Age	-0.045***	[-0.055, -0.035]	-0.032***	[-0.045, -0.020]
Birth Tally	0.0053***	[0.0026, 0.0080]	0.0079***	[0.0042, 0.012]
Fixed Effect – Month & Phase	Yes		Yes	
Number of Observations	44,735		26,299	
R ²	0.12		0.081	
Outlying Predictions Count	553		447	

* p < 0.10, ** p < 0.05, *** p < 0.01

Table 29. LPM Results: Wasted - NDVI

Linear Probability Model	Nigeria		Wasted	Kenya
<i>Average Marginal Effects with 95% Confidence Interval in Brackets</i>				
Sex – Female	-0.015***	[-0.021, -0.0081]	-0.012***	[-0.018, -0.0064]
Delivery – Clinic	-0.020***	[-0.028, -0.012]	-0.015***	[-0.022, -0.0087]
Birth – Singleton	-0.046***	[-0.066, -0.025]	-0.046***	[-0.068, -0.024]
Weaned – By 1 Year Old	-0.0039	[-0.011, 0.0036]	-0.0038	[-0.012, 0.0045]
Vaccines – Minimum	-0.019***	[-0.028, -0.010]	-0.026**	[-0.046, -0.0059]
Vaccines – Maximum	-0.0061	[-0.015, 0.0027]	-0.0025	[-0.0086, 0.0036]
Diet – Diverse	0.0016	[-0.0061, 0.0094]	-0.0080*	[-0.016, 0.00042]
Sick – Asymptomatic	-0.014***	[-0.022, -0.0065]	-0.000019	[-0.0058, 0.0058]
Latrine – Improved	0.014***	[0.0056, 0.021]	0.0085**	[0.0011, 0.016]
Water – Improved	0.0096**	[0.0020, 0.017]	-0.0054	[-0.012, 0.0012]
Residence – Rural	-0.0073*	[-0.016, 0.0012]	0.0015	[-0.0060, 0.0091]
Mothers Education				
Primary	-0.030***	[-0.039, -0.020]	-0.049***	[-0.060, -0.038]
Secondary	-0.040***	[-0.051, -0.030]	-0.043***	[-0.056, -0.031]
Higher	-0.067***	[-0.083, -0.051]	-0.051***	[-0.065, -0.036]
Wealth Index				
Poorer	-0.0055	[-0.016, 0.0053]	-0.018***	[-0.027, -0.0090]
Middle	-0.021***	[-0.032, -0.0094]	-0.018***	[-0.027, -0.0079]
Richer	-0.021***	[-0.034, -0.0084]	-0.020***	[-0.030, -0.0094]
Richest	-0.021***	[-0.037, -0.0058]	-0.031***	[-0.043, -0.018]
Child’s Age	-0.024***	[-0.027, -0.020]	-0.0021	[-0.0051, 0.00089]
Mother’s Age	-0.0016	[-0.0090, 0.0059]	-0.0014	[-0.0083, 0.0054]
Birth Tally	-0.00092	[-0.0030, 0.0012]	0.00025	[-0.0019, 0.0024]
NDVI	-0.22***	[-0.25, -0.19]	-0.15***	[-0.18, -0.12]
NDVI Anomaly	0.26***	[0.13, 0.39]	0.19***	[0.090, 0.28]
Fixed Effect – Month & Phase	Yes		Yes	
Number of Observations	44,717		26,299	
R ²	0.050		0.045	
Outlying Predictions Count	838		1,068	

* p < 0.10, ** p < 0.05, *** p < 0.01

Table 30. LPM Results: Stunted - NDVI

Linear Probability Model	Nigeria		Kenya	
Average Marginal Effects with 95% Confidence Interval in Brackets				
Sex – Female	-0.043***	[-0.051, -0.034]	-0.069***	[-0.079, -0.058]
Delivery – Clinic	-0.040***	[-0.052, -0.029]	-0.045***	[-0.058, -0.032]
Birth – Singleton	-0.11***	[-0.13, -0.082]	-0.20***	[-0.23, -0.16]
Weaned – By 1 Year Old	-0.010*	[-0.021, 0.00012]	-0.016**	[-0.032, -0.00079]
Vaccines – Minimum	0.0069	[-0.0041, 0.018]	-0.018	[-0.045, 0.0094]
Vaccines – Maximum	-0.049***	[-0.062, -0.037]	-0.015**	[-0.027, -0.0023]
Diet – Diverse	-0.016***	[-0.026, -0.0056]	0.0010	[-0.015, 0.017]
Sick – Asymptomatic	-0.023***	[-0.033, -0.013]	-0.0091	[-0.020, 0.0017]
Latrine – Improved	0.0072	[-0.0028, 0.017]	-0.040***	[-0.054, -0.026]
Water – Improved	-0.0062	[-0.016, 0.0036]	-0.013**	[-0.025, -0.0010]
Residence – Rural	0.021***	[0.0097, 0.032]	-0.019**	[-0.034, -0.0042]
Mothers Education				
Primary	-0.045***	[-0.058, -0.033]	0.038***	[0.020, 0.055]
Secondary	-0.087***	[-0.10, -0.072]	-0.011	[-0.032, 0.011]
Higher	-0.13***	[-0.15, -0.11]	-0.029**	[-0.057, -0.0022]
Wealth Index				
Poorer	-0.023***	[-0.037, -0.0098]	-0.046***	[-0.063, -0.028]
Middle	-0.053***	[-0.068, -0.038]	-0.083***	[-0.10, -0.064]
Richer	-0.10***	[-0.12, -0.087]	-0.095***	[-0.12, -0.075]
Richest	-0.14***	[-0.16, -0.12]	-0.13***	[-0.16, -0.11]
Child’s Age	-0.0046**	[-0.0091, -0.00017]	-0.023***	[-0.029, -0.018]
Mother’s Age	-0.036***	[-0.046, -0.026]	-0.032***	[-0.044, -0.019]
Birth Tally	0.0034**	[0.00071, 0.0061]	0.0076***	[0.0038, 0.011]
NDVI	-0.31***	[-0.35, -0.27]	0.18***	[0.13, 0.22]
NDVI Anomaly	0.61***	[0.45, 0.78]	-0.048	[-0.22, 0.12]
Fixed Effect – Month & Phase	Yes		Yes	
Number of Observations	44,717		26,299	
R ²	0.12		0.083	
Outlying Predictions Count	668		471	

* p < 0.10, ** p < 0.05, *** p < 0.01

Table 31. LPM Results: Wasted - Precipitation

Linear Probability Model	Nigeria		Kenya	
<i>Average Marginal Effects with 95% Confidence Interval in Brackets</i>				
Sex – Female	-0.014***	[-0.021, -0.0076]	-0.012***	[-0.018, -0.0067]
Delivery – Clinic	-0.028***	[-0.036, -0.020]	-0.018***	[-0.025, -0.012]
Birth – Singleton	-0.042***	[-0.063, -0.022]	-0.046***	[-0.069, -0.024]
Weaned – By 1 Year Old	-0.0035	[-0.011, 0.0040]	-0.0043	[-0.013, 0.0040]
Vaccines – Minimum	-0.021***	[-0.030, -0.012]	-0.027***	[-0.047, -0.0074]
Vaccines – Maximum	-0.0078*	[-0.017, 0.00098]	-0.0041	[-0.010, 0.0020]
Diet – Diverse	0.0012	[-0.0066, 0.0090]	-0.010**	[-0.019, -0.0018]
Sick – Asymptomatic	-0.017***	[-0.024, -0.0088]	0.00047	[-0.0053, 0.0063]
Latrine – Improved	0.020***	[0.012, 0.028]	0.0069*	[-0.00046, 0.014]
Water – Improved	0.012***	[0.0046, 0.020]	-0.0032	[-0.0098, 0.0035]
Residence – Rural	-0.013***	[-0.022, -0.0045]	-0.0049	[-0.012, 0.0025]
Mothers Education				
Primary	-0.035***	[-0.044, -0.025]	-0.061***	[-0.071, -0.050]
Secondary	-0.044***	[-0.055, -0.034]	-0.055***	[-0.067, -0.043]
Higher	-0.072***	[-0.088, -0.056]	-0.062***	[-0.076, -0.047]
Wealth Index				
Poorer	-0.0079	[-0.019, 0.0030]	-0.022***	[-0.031, -0.012]
Middle	-0.026***	[-0.037, -0.014]	-0.021***	[-0.030, -0.011]
Richer	-0.026***	[-0.039, -0.013]	-0.024***	[-0.034, -0.013]
Richest	-0.018**	[-0.034, -0.0025]	-0.032***	[-0.045, -0.020]
Child’s Age	-0.024***	[-0.027, -0.020]	-0.0024	[-0.0054, 0.00065]
Mother’s Age	-0.0048	[-0.012, 0.0026]	-0.0028	[-0.0098, 0.0041]
Birth Tally	0.000052	[-0.0021, 0.0022]	0.00066	[-0.0015, 0.0029]
Precipitation	-0.026***	[-0.030, -0.021]	-0.022***	[-0.027, -0.017]
Precipitation Anomaly	0.0070	[-0.0054, 0.019]	0.017*	[-0.0013, 0.035]
Fixed Effect – Month & Phase	Yes		Yes	
Number of Observations	44,717		26,299	
R ²	0.047		0.041	
Outlying Predictions Count	879		976	

* p < 0.10, ** p < 0.05, *** p < 0.01

Table 32. LPM Results: Stunted - Precipitation

Linear Probability Model	Nigeria		Stunted		Kenya
Average Marginal Effects with 95% Confidence Interval in Brackets					
Sex – Female	-0.042***	[-0.051, -0.034]	-0.068***		[-0.079, -0.058]
Delivery – Clinic	-0.052***	[-0.064, -0.041]	-0.043***		[-0.056, -0.030]
Birth – Singleton	-0.10***	[-0.13, -0.078]	-0.20***		[-0.23, -0.16]
Weaned – By 1 Year Old	-0.0082	[-0.019, 0.0022]	-0.016**		[-0.031, -0.0000091]
Vaccines – Minimum	0.0051	[-0.0058, 0.016]	-0.015		[-0.043, 0.012]
Vaccines – Maximum	-0.050***	[-0.063, -0.038]	-0.013**		[-0.025, -0.00042]
Diet – Diverse	-0.015***	[-0.025, -0.0050]	0.0032		[-0.013, 0.019]
Sick – Asymptomatic	-0.026***	[-0.036, -0.016]	-0.0099*		[-0.021, 0.00096]
Latrine – Improved	0.012**	[0.0017, 0.021]	-0.039***		[-0.053, -0.025]
Water – Improved	-0.0072	[-0.017, 0.0026]	-0.015**		[-0.027, -0.0029]
Residence – Rural	0.019***	[0.0079, 0.031]	-0.010		[-0.025, 0.0044]
Mothers Education					
Primary	-0.041***	[-0.054, -0.029]	0.055***		[0.038, 0.072]
Secondary	-0.077***	[-0.091, -0.062]	0.0071		[-0.014, 0.028]
Higher	-0.12***	[-0.15, -0.10]	-0.012		[-0.039, 0.015]
Wealth Index					
Poorer	-0.021***	[-0.035, -0.0073]	-0.041***		[-0.059, -0.024]
Middle	-0.049***	[-0.064, -0.033]	-0.078***		[-0.097, -0.060]
Richer	-0.099***	[-0.12, -0.081]	-0.090***		[-0.11, -0.069]
Richest	-0.13***	[-0.15, -0.11]	-0.13***		[-0.15, -0.11]
Child’s Age	-0.0048**	[-0.0092, -0.00030]	-0.023***		[-0.029, -0.017]
Mother’s Age	-0.039***	[-0.048, -0.029]	-0.031***		[-0.043, -0.018]
Birth Tally	0.0047***	[0.0020, 0.0074]	0.0074***		[0.0036, 0.011]
Precipitation	-0.059***	[-0.065, -0.052]	0.014***		[0.0042, 0.024]
Precipitation Anomaly	0.041***	[0.025, 0.058]	-0.020		[-0.059, 0.018]
Fixed Effect – Month & Phase	Yes		Yes		
Number of Observations	44,717		26,299		
R ²	0.12		0.082		
Outlying Predictions Count	673		443		

* p < 0.10, ** p < 0.05, *** p < 0.01

Table 33. LPM Results: Wasted - Temperature

Linear Probability Model	Wasted			
	Nigeria		Kenya	
Average Marginal Effects with 95% Confidence Interval in Brackets				
Sex – Female	-0.014***	[-0.021, -0.0076]	-0.012***	[-0.018, -0.0065]
Delivery – Clinic	-0.019***	[-0.027, -0.011]	-0.017***	[-0.023, -0.0100]
Birth – Singleton	-0.045***	[-0.066, -0.025]	-0.046***	[-0.068, -0.023]
Weaned – By 1 Year Old	-0.0027	[-0.010, 0.0048]	-0.0036	[-0.012, 0.0047]
Vaccines – Minimum	-0.015***	[-0.024, -0.0060]	-0.027***	[-0.046, -0.0066]
Vaccines – Maximum	-0.0046	[-0.013, 0.0041]	-0.0028	[-0.0089, 0.0034]
Diet – Diverse	0.0018	[-0.0060, 0.0095]	-0.0078*	[-0.016, 0.00067]
Sick – Asymptomatic	-0.014***	[-0.022, -0.0063]	0.0028	[-0.0031, 0.0086]
Latrine – Improved	0.0098**	[0.0018, 0.018]	0.0075**	[0.000085, 0.015]
Water – Improved	0.0071*	[-0.00059, 0.015]	-0.0057*	[-0.012, 0.00097]
Residence – Rural	-0.013***	[-0.021, -0.0043]	-0.0032	[-0.011, 0.0042]
Mothers Education				
Primary	-0.023***	[-0.033, -0.014]	-0.065***	[-0.076, -0.055]
Secondary	-0.033***	[-0.044, -0.023]	-0.060***	[-0.072, -0.048]
Higher	-0.060***	[-0.076, -0.044]	-0.068***	[-0.082, -0.054]
Wealth Index				
Poorer	-0.00019	[-0.011, 0.011]	-0.022***	[-0.031, -0.013]
Middle	-0.014**	[-0.026, -0.0025]	-0.019***	[-0.029, -0.0096]
Richer	-0.014**	[-0.027, -0.0011]	-0.020***	[-0.031, -0.0097]
Richest	-0.0045	[-0.020, 0.011]	-0.025***	[-0.038, -0.013]
Child’s Age	-0.024***	[-0.027, -0.020]	-0.0022	[-0.0053, 0.00078]
Mother’s Age	0.00090	[-0.0066, 0.0084]	0.0012	[-0.0058, 0.0081]
Birth Tally	-0.0015	[-0.0036, 0.00064]	-0.00076	[-0.0030, 0.0014]
Temperature	0.017***	[0.015, 0.019]	0.0032***	[0.0022, 0.0041]
Temperature Anomaly	-0.038***	[-0.051, -0.026]	-0.0017	[-0.0095, 0.0060]
Fixed Effect – Month & Phase	Yes		Yes	
Number of Observations	44,717		26,299	
R ²	0.052		0.041	
Outlying Predictions Count	716		707	

* p < 0.10, ** p < 0.05, *** p < 0.01

Table 34. LPM Results: Stunted - Temperature

Linear Probability Model	Stunted			
	Nigeria		Kenya	
Average Marginal Effects with 95% Confidence Interval in Brackets				
Sex – Female	-0.042***	[-0.051, -0.034]	-0.069***	[-0.079, -0.058]
Delivery – Clinic	-0.045***	[-0.056, -0.034]	-0.044***	[-0.057, -0.031]
Birth – Singleton	-0.10***	[-0.13, -0.079]	-0.20***	[-0.24, -0.16]
Weaned – By 1 Year Old	-0.010*	[-0.020, 0.00048]	-0.018**	[-0.034, -0.0026]
Vaccines – Minimum	0.0090	[-0.0020, 0.020]	-0.020	[-0.047, 0.0076]
Vaccines – Maximum	-0.051***	[-0.063, -0.038]	-0.015**	[-0.027, -0.0028]
Diet – Diverse	-0.017***	[-0.027, -0.0066]	-0.0021	[-0.018, 0.014]
Sick – Asymptomatic	-0.023***	[-0.033, -0.014]	-0.013**	[-0.024, -0.0019]
Latrine – Improved	0.012**	[0.0022, 0.022]	-0.040***	[-0.054, -0.026]
Water – Improved	-0.0031	[-0.013, 0.0068]	-0.011*	[-0.024, 0.00086]
Residence – Rural	0.0090	[-0.0023, 0.020]	-0.017**	[-0.032, -0.0029]
Mothers Education				
Primary	-0.053***	[-0.066, -0.040]	0.045***	[0.028, 0.061]
Secondary	-0.096***	[-0.11, -0.081]	-0.0063	[-0.027, 0.015]
Higher	-0.14***	[-0.16, -0.12]	-0.023*	[-0.050, 0.0035]
Wealth Index				
Poorer	-0.024***	[-0.038, -0.011]	-0.046***	[-0.064, -0.029]
Middle	-0.058***	[-0.073, -0.043]	-0.087***	[-0.11, -0.068]
Richer	-0.11***	[-0.13, -0.092]	-0.10***	[-0.12, -0.082]
Richest	-0.14***	[-0.16, -0.12]	-0.15***	[-0.17, -0.12]
Child’s Age	-0.0047**	[-0.0092, -0.00021]	-0.023***	[-0.029, -0.018]
Mother’s Age	-0.038***	[-0.048, -0.029]	-0.037***	[-0.049, -0.024]
Birth Tally	0.0038***	[0.0011, 0.0066]	0.0094***	[0.0056, 0.013]
Temperature	0.013***	[0.011, 0.016]	-0.0079***	[-0.0095, -0.0063]
Temperature Anomaly	-0.032***	[-0.048, -0.017]	0.00080	[-0.012, 0.014]
Fixed Effect – Month & Phase	Yes		Yes	
Number of Observations	44,717		26,299	
R ²	0.12		0.085	
Outlying Predictions Count	599		493	

* p < 0.10, ** p < 0.05, *** p < 0.01

7.3.10.3 Logit Model Results

Table 35. Logit Results: Wasted - Base

Logit Model	Wasted			
	Nigeria		Kenya	
Average Marginal Effects with 95% Confidence Interval in Brackets				
Sex – Female	-0.014***	[-0.021, -0.0076]	-0.013***	[-0.018, -0.0068]
Delivery – Clinic	-0.032***	[-0.040, -0.024]	-0.018***	[-0.025, -0.011]
Birth – Singleton	-0.044***	[-0.067, -0.022]	-0.048***	[-0.072, -0.024]
Weaned – By 1 Year Old	-0.0059	[-0.014, 0.0022]	-0.0036	[-0.011, 0.0040]
Vaccines – Minimum	-0.018***	[-0.026, -0.010]	-0.013**	[-0.026, -0.0010]
Vaccines – Maximum	-0.014**	[-0.025, -0.0033]	-0.0059*	[-0.013, 0.00079]
Diet – Diverse	0.000013	[-0.0080, 0.0081]	-0.0072*	[-0.015, 0.00088]
Sick – Asymptomatic	-0.016***	[-0.024, -0.0084]	0.0032	[-0.0027, 0.0090]
Latrine – Improved	0.024***	[0.016, 0.032]	0.0092*	[-0.00079, 0.019]
Water – Improved	0.015***	[0.0081, 0.023]	-0.0034	[-0.0097, 0.0030]
Residence – Rural	-0.022***	[-0.031, -0.012]	-0.0095**	[-0.019, -0.00047]
Mothers Education				
Primary	-0.047***	[-0.056, -0.038]	-0.060***	[-0.070, -0.051]
Secondary	-0.062***	[-0.072, -0.052]	-0.059***	[-0.071, -0.047]
Higher	-0.090***	[-0.11, -0.074]	-0.071***	[-0.086, -0.056]
Wealth Index				
Poorer	-0.012**	[-0.022, -0.0023]	-0.025***	[-0.034, -0.016]
Middle	-0.035***	[-0.046, -0.024]	-0.024***	[-0.034, -0.013]
Richer	-0.035***	[-0.048, -0.022]	-0.026***	[-0.038, -0.015]
Richest	-0.024***	[-0.041, -0.0076]	-0.032***	[-0.045, -0.019]
Child’s Age	-0.026***	[-0.029, -0.022]	-0.0024	[-0.0055, 0.00073]
Mother’s Age	-0.0080**	[-0.016, -0.00026]	-0.0022	[-0.0090, 0.0046]
Birth Tally	0.00037	[-0.0017, 0.0025]	0.00038	[-0.0016, 0.0023]
Fixed Effect – Month & Phase	Yes		Yes	
Number of Observations	44,735		26,299	
Log Pseudo Likelihood	-18,287.02		-5,760.24	
Pseudo R ²	0.053		0.072	
Pearson’s χ^2 <i>p</i> -Value	0.341		0.753	

* p < 0.10, ** p < 0.05, *** p < 0.01

Table 36. Logit Results: Stunted - Base

Logit Model	Nigeria		Kenya	
Average Marginal Effects with 95% Confidence Interval in Brackets				
Sex – Female	-0.043***	[-0.051, -0.034]	-0.069***	[-0.079, -0.058]
Delivery – Clinic	-0.053***	[-0.064, -0.042]	-0.042***	[-0.055, -0.030]
Birth – Singleton	-0.10***	[-0.13, -0.077]	-0.20***	[-0.24, -0.16]
Weaned – By 1 Year Old	-0.0074	[-0.018, 0.0031]	-0.0092	[-0.025, 0.0063]
Vaccines – Minimum	0.0014	[-0.0089, 0.012]	-0.019	[-0.045, 0.0074]
Vaccines – Maximum	-0.051***	[-0.064, -0.037]	-0.010*	[-0.022, 0.0016]
Diet – Diverse	-0.019***	[-0.029, -0.0085]	0.0026	[-0.013, 0.018]
Sick – Asymptomatic	-0.024***	[-0.034, -0.014]	-0.011**	[-0.022, -0.00065]
Latrine – Improved	0.025***	[0.015, 0.035]	-0.042***	[-0.057, -0.027]
Water – Improved	0.0044	[-0.0051, 0.014]	-0.014**	[-0.025, -0.0019]
Residence – Rural	0.0046	[-0.0071, 0.016]	-0.0095	[-0.025, 0.0057]
Mothers Education				
Primary	-0.068***	[-0.080, -0.056]	0.059***	[0.045, 0.074]
Secondary	-0.12***	[-0.13, -0.10]	0.0084	[-0.011, 0.028]
Higher	-0.18***	[-0.21, -0.16]	-0.030*	[-0.060, 0.00080]
Wealth Index				
Poorer	-0.033***	[-0.046, -0.019]	-0.036***	[-0.053, -0.019]
Middle	-0.072***	[-0.086, -0.057]	-0.072***	[-0.090, -0.054]
Richer	-0.12***	[-0.14, -0.11]	-0.086***	[-0.11, -0.065]
Richest	-0.16***	[-0.18, -0.14]	-0.13***	[-0.16, -0.11]
Child’s Age	-0.0046**	[-0.0088, -0.00029]	-0.021***	[-0.027, -0.016]
Mother’s Age	-0.046***	[-0.056, -0.036]	-0.033***	[-0.046, -0.021]
Birth Tally	0.0059***	[0.0032, 0.0086]	0.0080***	[0.0043, 0.012]
Fixed Effect – Month & Phase	Yes		Yes	
Number of Observations	44,735		26,299	
Log Pseudo Likelihood	-27,099.77		-14,756.69	
Pseudo R ²	0.095		0.072	
Pearson’s χ^2 <i>p</i> -Value	0.061		0.192	

* p < 0.10, ** p < 0.05, *** p < 0.01

Table 37. Logit Results: Wasted - NDVI

Logit Model	Nigeria		Wasted		Kenya
Average Marginal Effects with 95% Confidence Interval in Brackets					
Sex – Female	-0.015***	[-0.021, -0.0080]	-0.012***		[-0.018, -0.0063]
Delivery – Clinic	-0.023***	[-0.032, -0.015]	-0.017***		[-0.024, -0.0096]
Birth – Singleton	-0.050***	[-0.072, -0.027]	-0.049***		[-0.073, -0.025]
Weaned – By 1 Year Old	-0.0047	[-0.013, 0.0034]	-0.0025		[-0.010, 0.0049]
Vaccines – Minimum	-0.016***	[-0.024, -0.0079]	-0.010*		[-0.022, 0.0018]
Vaccines – Maximum	-0.010*	[-0.021, 0.00090]	-0.0049		[-0.012, 0.0018]
Diet – Diverse	0.0021	[-0.0060, 0.010]	-0.0045		[-0.013, 0.0039]
Sick – Asymptomatic	-0.014***	[-0.022, -0.0067]	0.0011		[-0.0047, 0.0069]
Latrine – Improved	0.012***	[0.0044, 0.020]	0.012**		[0.0015, 0.022]
Water – Improved	0.0083**	[0.00089, 0.016]	-0.0048		[-0.011, 0.0015]
Residence – Rural	-0.010**	[-0.020, -0.00081]	-0.0026		[-0.011, 0.0062]
Mothers Education					
Primary	-0.028***	[-0.038, -0.019]	-0.035***		[-0.045, -0.026]
Secondary	-0.041***	[-0.052, -0.030]	-0.033***		[-0.045, -0.020]
Higher	-0.072***	[-0.089, -0.055]	-0.046***		[-0.062, -0.030]
Wealth Index					
Poorer	-0.0035	[-0.013, 0.0062]	-0.019***		[-0.029, -0.010]
Middle	-0.018***	[-0.029, -0.0072]	-0.018***		[-0.029, -0.0079]
Richer	-0.020***	[-0.033, -0.0067]	-0.021***		[-0.032, -0.0092]
Richest	-0.016*	[-0.033, 0.00038]	-0.029***		[-0.042, -0.016]
Child’s Age	-0.026***	[-0.029, -0.022]	-0.0022		[-0.0053, 0.00088]
Mother’s Age	-0.0018	[-0.0095, 0.0060]	-0.0035		[-0.010, 0.0033]
Birth Tally	-0.00096	[-0.0031, 0.0011]	0.00094		[-0.00099, 0.0029]
NDVI	-0.21***	[-0.24, -0.18]	-0.11***		[-0.13, -0.087]
NDVI Anomaly	0.20***	[0.069, 0.33]	0.21***		[0.13, 0.30]
Fixed Effect – Month & Phase	Yes		Yes		
Number of Observations	44,717		26,299		
Log Pseudo Likelihood	-18,172.69		-5,700.61		
Pseudo R ²	0.059		0.081		
Pearson’s χ^2 <i>p</i> -Value	0.596		0.903		

* p < 0.10, ** p < 0.05, *** p < 0.01

Table 38. Logit Results: Stunted - NDVI

Logit Model	Nigeria		Kenya	
<i>Average Marginal Effects with 95% Confidence Interval in Brackets</i>				
Sex – Female	-0.043***	[-0.051, -0.035]	-0.069***	[-0.080, -0.059]
Delivery – Clinic	-0.040***	[-0.051, -0.028]	-0.044***	[-0.056, -0.031]
Birth – Singleton	-0.11***	[-0.14, -0.085]	-0.20***	[-0.24, -0.16]
Weaned – By 1 Year Old	-0.0056	[-0.016, 0.0049]	-0.011	[-0.026, 0.0049]
Vaccines – Minimum	0.0047	[-0.0057, 0.015]	-0.022*	[-0.048, 0.0040]
Vaccines – Maximum	-0.044***	[-0.057, -0.030]	-0.012*	[-0.024, 0.000023]
Diet – Diverse	-0.016***	[-0.027, -0.0059]	0.00020	[-0.015, 0.016]
Sick – Asymptomatic	-0.023***	[-0.032, -0.013]	-0.0095*	[-0.020, 0.0013]
Latrine – Improved	0.0064	[-0.0037, 0.016]	-0.044***	[-0.059, -0.029]
Water – Improved	-0.0071	[-0.017, 0.0025]	-0.012**	[-0.024, -0.00074]
Residence – Rural	0.021***	[0.0096, 0.033]	-0.019**	[-0.035, -0.0038]
Mothers Education				
Primary	-0.039***	[-0.051, -0.027]	0.035***	[0.018, 0.051]
Secondary	-0.084***	[-0.099, -0.070]	-0.017	[-0.039, 0.0039]
Higher	-0.15***	[-0.18, -0.13]	-0.055***	[-0.086, -0.024]
Wealth Index				
Poorer	-0.020***	[-0.033, -0.0066]	-0.043***	[-0.060, -0.026]
Middle	-0.047***	[-0.061, -0.032]	-0.078***	[-0.096, -0.060]
Richer	-0.098***	[-0.12, -0.081]	-0.092***	[-0.11, -0.071]
Richest	-0.15***	[-0.17, -0.12]	-0.14***	[-0.16, -0.11]
Child’s Age	-0.0047**	[-0.0089, -0.00041]	-0.022***	[-0.027, -0.016]
Mother’s Age	-0.036***	[-0.046, -0.027]	-0.033***	[-0.046, -0.020]
Birth Tally	0.0039***	[0.0012, 0.0066]	0.0076***	[0.0040, 0.011]
NDVI	-0.31***	[-0.34, -0.27]	0.17***	[0.13, 0.22]
NDVI Anomaly	0.63***	[0.46, 0.80]	-0.044	[-0.21, 0.12]
Fixed Effect – Month & Phase	Yes		Yes	
Number of Observations	44,717		26,299	
Log Pseudo Likelihood	-26,929.20		-14,729.00	
Pseudo R ²	0.100		0.074	
Pearson’s χ^2 <i>p</i> -Value	0.058		0.175	

* p < 0.10, ** p < 0.05, *** p < 0.01

Table 39. Logit Results: Wasted - Precipitation

Logit Model	Wasted			
	Nigeria		Kenya	
Average Marginal Effects with 95% Confidence Interval in Brackets				
Sex – Female	-0.014***	[-0.021, -0.0076]	-0.012***	[-0.018, -0.0067]
Delivery – Clinic	-0.030***	[-0.038, -0.021]	-0.019***	[-0.027, -0.012]
Birth – Singleton	-0.046***	[-0.069, -0.024]	-0.050***	[-0.074, -0.026]
Weaned – By 1 Year Old	-0.0044	[-0.012, 0.0037]	-0.0031	[-0.011, 0.0044]
Vaccines – Minimum	-0.018***	[-0.026, -0.0098]	-0.011*	[-0.023, 0.0011]
Vaccines – Maximum	-0.011**	[-0.022, -0.00012]	-0.0060*	[-0.013, 0.00076]
Diet – Diverse	0.0020	[-0.0061, 0.010]	-0.0071*	[-0.015, 0.0011]
Sick – Asymptomatic	-0.017***	[-0.024, -0.0090]	0.0010	[-0.0048, 0.0069]
Latrine – Improved	0.018***	[0.010, 0.025]	0.0098*	[-0.00028, 0.020]
Water – Improved	0.010***	[0.0029, 0.018]	-0.0031	[-0.0094, 0.0032]
Residence – Rural	-0.015***	[-0.025, -0.0060]	-0.0069	[-0.016, 0.0020]
Mothers Education				
Primary	-0.032***	[-0.041, -0.022]	-0.042***	[-0.051, -0.033]
Secondary	-0.043***	[-0.054, -0.032]	-0.039***	[-0.051, -0.026]
Higher	-0.075***	[-0.091, -0.058]	-0.052***	[-0.068, -0.036]
Wealth Index				
Poorer	-0.0057	[-0.015, 0.0041]	-0.021***	[-0.031, -0.012]
Middle	-0.023***	[-0.034, -0.011]	-0.020***	[-0.031, -0.0100]
Richer	-0.024***	[-0.037, -0.010]	-0.024***	[-0.035, -0.013]
Richest	-0.013	[-0.030, 0.0040]	-0.032***	[-0.044, -0.019]
Child’s Age	-0.026***	[-0.029, -0.022]	-0.0024	[-0.0055, 0.00076]
Mother’s Age	-0.0042	[-0.012, 0.0035]	-0.0038	[-0.011, 0.0030]
Birth Tally	-0.00015	[-0.0023, 0.0020]	0.0011	[-0.00089, 0.0030]
Precipitation	-0.029***	[-0.034, -0.023]	-0.025***	[-0.031, -0.019]
Precipitation Anomaly	0.0024	[-0.012, 0.016]	0.028**	[0.0054, 0.051]
Fixed Effect – Month & Phase	Yes		Yes	
Number of Observations	44,717		26,299	
Log Pseudo Likelihood	-18,225.72		-5,723.46	
Pseudo R ²	0.056		0.078	
Pearson’s χ^2 <i>p</i> -Value	0.343		0.620	

* p < 0.10, ** p < 0.05, *** p < 0.01

Table 40. Logit Results: Stunted - Precipitation

Logit Model	Stunted			
	Nigeria		Kenya	
Average Marginal Effects with 95% Confidence Interval in Brackets				
Sex – Female	-0.042***	[-0.051, -0.034]	-0.069***	[-0.079, -0.058]
Delivery – Clinic	-0.052***	[-0.063, -0.041]	-0.041***	[-0.054, -0.029]
Birth – Singleton	-0.11***	[-0.13, -0.081]	-0.20***	[-0.24, -0.16]
Weaned – By 1 Year Old	-0.0033	[-0.014, 0.0072]	-0.0097	[-0.025, 0.0058]
Vaccines – Minimum	0.0031	[-0.0072, 0.013]	-0.020	[-0.046, 0.0064]
Vaccines – Maximum	-0.045***	[-0.059, -0.032]	-0.0099	[-0.022, 0.0019]
Diet – Diverse	-0.016***	[-0.026, -0.0058]	0.0027	[-0.013, 0.018]
Sick – Asymptomatic	-0.025***	[-0.035, -0.016]	-0.010*	[-0.021, 0.00063]
Latrine – Improved	0.010**	[0.00025, 0.020]	-0.042***	[-0.057, -0.027]
Water – Improved	-0.0083*	[-0.018, 0.0013]	-0.014**	[-0.026, -0.0027]
Residence – Rural	0.019***	[0.0071, 0.031]	-0.011	[-0.026, 0.0043]
Mothers Education				
Primary	-0.034***	[-0.046, -0.022]	0.052***	[0.036, 0.067]
Secondary	-0.073***	[-0.088, -0.058]	-0.00074	[-0.022, 0.020]
Higher	-0.15***	[-0.17, -0.12]	-0.039**	[-0.070, -0.0081]
Wealth Index				
Poorer	-0.016**	[-0.029, -0.0033]	-0.038***	[-0.055, -0.021]
Middle	-0.041***	[-0.055, -0.026]	-0.074***	[-0.093, -0.056]
Richer	-0.091***	[-0.11, -0.074]	-0.087***	[-0.11, -0.066]
Richest	-0.13***	[-0.15, -0.11]	-0.13***	[-0.16, -0.11]
Child’s Age	-0.0049**	[-0.0091, -0.00064]	-0.021***	[-0.027, -0.016]
Mother’s Age	-0.039***	[-0.049, -0.029]	-0.032***	[-0.045, -0.019]
Birth Tally	0.0051***	[0.0024, 0.0078]	0.0075***	[0.0038, 0.011]
Precipitation	-0.060***	[-0.067, -0.054]	0.014***	[0.0041, 0.024]
Precipitation Anomaly	0.053***	[0.035, 0.071]	-0.020	[-0.059, 0.019]
Fixed Effect – Month & Phase	Yes		Yes	
Number of Observations	44,717		26,299	
Log Pseudo Likelihood	-26,909.39		-14,752.76	
Pseudo R ²	0.101		0.073	
Pearson’s χ^2 <i>p</i> -Value	0.050		0.191	

* p < 0.10, ** p < 0.05, *** p < 0.01

Table 41. Logit Results: Wasted - Temperature

Logit Model	Nigeria		Kenya	
Average Marginal Effects with 95% Confidence Interval in Brackets				
Sex – Female	-0.014***	[-0.021, -0.0076]	-0.012***	[-0.018, -0.0064]
Delivery – Clinic	-0.022***	[-0.031, -0.013]	-0.018***	[-0.025, -0.011]
Birth – Singleton	-0.049***	[-0.071, -0.026]	-0.048***	[-0.072, -0.024]
Weaned – By 1 Year Old	-0.0037	[-0.012, 0.0043]	-0.0027	[-0.010, 0.0048]
Vaccines – Minimum	-0.012***	[-0.020, -0.0044]	-0.011*	[-0.023, 0.0014]
Vaccines – Maximum	-0.0085	[-0.019, 0.0026]	-0.0051	[-0.012, 0.0017]
Diet – Diverse	0.0021	[-0.0060, 0.010]	-0.0049	[-0.013, 0.0034]
Sick – Asymptomatic	-0.014***	[-0.022, -0.0064]	0.0031	[-0.0028, 0.0090]
Latrine – Improved	0.0093**	[0.0016, 0.017]	0.010**	[0.000074, 0.020]
Water – Improved	0.0052	[-0.0022, 0.013]	-0.0051	[-0.011, 0.0012]
Residence – Rural	-0.015***	[-0.024, -0.0053]	-0.0046	[-0.013, 0.0042]
Mothers Education				
Primary	-0.024***	[-0.033, -0.014]	-0.049***	[-0.059, -0.040]
Secondary	-0.036***	[-0.047, -0.025]	-0.046***	[-0.059, -0.034]
Higher	-0.067***	[-0.084, -0.050]	-0.059***	[-0.075, -0.043]
Wealth Index				
Poorer	0.00088	[-0.0087, 0.010]	-0.022***	[-0.031, -0.012]
Middle	-0.012**	[-0.023, -0.0013]	-0.019***	[-0.029, -0.0086]
Richer	-0.013*	[-0.026, 0.00014]	-0.021***	[-0.032, -0.0094]
Richest	-0.00046	[-0.018, 0.017]	-0.025***	[-0.039, -0.012]
Child’s Age	-0.026***	[-0.030, -0.022]	-0.0023	[-0.0055, 0.00080]
Mother’s Age	0.00051	[-0.0072, 0.0083]	-0.00077	[-0.0076, 0.0060]
Birth Tally	-0.0014	[-0.0035, 0.00073]	-0.000034	[-0.0020, 0.0019]
Temperature	0.015***	[0.013, 0.017]	0.0031***	[0.0021, 0.0040]
Temperature Anomaly	-0.029***	[-0.042, -0.017]	-0.0034	[-0.010, 0.0033]
Fixed Effect – Month & Phase	Yes		Yes	
Number of Observations	44,717		26,299	
Log Pseudo Likelihood	-18,142.48		-5,736.00	
Pseudo R ²	0.060		0.076	
Pearson’s χ^2 <i>p</i> -Value	0.707		0.698	

* p < 0.10, ** p < 0.05, *** p < 0.01

Table 42. Logit Results: Stunted - Temperature

Logit Model	Stunted			
	Nigeria		Kenya	
<i>Average Marginal Effects with 95% Confidence Interval in Brackets</i>				
Sex – Female	-0.042***	[-0.051, -0.034]	-0.069***	[-0.080, -0.059]
Delivery – Clinic	-0.045***	[-0.057, -0.034]	-0.043***	[-0.055, -0.030]
Birth – Singleton	-0.11***	[-0.13, -0.081]	-0.20***	[-0.24, -0.16]
Weaned – By 1 Year Old	-0.0054	[-0.016, 0.0051]	-0.013	[-0.028, 0.0029]
Vaccines – Minimum	0.0066	[-0.0038, 0.017]	-0.024*	[-0.050, 0.0021]
Vaccines – Maximum	-0.046***	[-0.059, -0.033]	-0.012**	[-0.024, -0.00048]
Diet – Diverse	-0.017***	[-0.028, -0.0070]	-0.0028	[-0.018, 0.013]
Sick – Asymptomatic	-0.023***	[-0.033, -0.013]	-0.013**	[-0.024, -0.0026]
Latrine – Improved	0.013**	[0.0024, 0.023]	-0.044***	[-0.059, -0.029]
Water – Improved	-0.0035	[-0.013, 0.0061]	-0.010*	[-0.022, 0.0016]
Residence – Rural	0.0098	[-0.0019, 0.021]	-0.018**	[-0.034, -0.0032]
Mothers Education				
Primary	-0.048***	[-0.060, -0.035]	0.042***	[0.027, 0.057]
Secondary	-0.095***	[-0.11, -0.080]	-0.013	[-0.034, 0.0077]
Higher	-0.16***	[-0.19, -0.14]	-0.048***	[-0.079, -0.018]
Wealth Index				
Poorer	-0.021***	[-0.035, -0.0081]	-0.045***	[-0.062, -0.028]
Middle	-0.053***	[-0.068, -0.038]	-0.084***	[-0.10, -0.066]
Richer	-0.10***	[-0.12, -0.087]	-0.10***	[-0.12, -0.080]
Richest	-0.14***	[-0.16, -0.12]	-0.15***	[-0.17, -0.13]
Child’s Age	-0.0048**	[-0.0091, -0.00053]	-0.022***	[-0.027, -0.016]
Mother’s Age	-0.039***	[-0.049, -0.030]	-0.038***	[-0.051, -0.025]
Birth Tally	0.0045***	[0.0018, 0.0072]	0.0094***	[0.0057, 0.013]
Temperature	0.012***	[0.0099, 0.015]	-0.0080***	[-0.0096, -0.0064]
Temperature Anomaly	-0.035***	[-0.051, -0.020]	-0.00011	[-0.013, 0.013]
Fixed Effect – Month & Phase	Yes		Yes	
Number of Observations	44,717		26,299	
Log Pseudo Likelihood	-27,024.73		-14,708.71	
Pseudo R ²	0.097		0.075	
Pearson’s χ^2 <i>p</i> -Value	0.082		0.183	

* p < 0.10, ** p < 0.05, *** p < 0.01

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