An Analysis Of Breastfeeding Behaviour, Health Care Practices, And Infant Survival With Emphasis On Reverse Causation: Evidence From Nigeria

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1 INTRODUCTION

The demographic and public health communities generally agree that breastfeeding provides early protection against disease for infants in environments where the alternatives to breast milk can easily be contaminated by poor household conditions[1]. Factors specific to the mother and child, such as mother and child's health status at the time of delivery, can influence the choice to initiate breastfeeding and its duration[2]. Reverse causation is said to occur when the infant is too sick to suckle, so that death leads to weaning rather than the reverse[3]. It arises from a child's inability to initiate or sustain breastfeeding and can therefore be complicated by the child's initial health status. Two mutually reinforcing reflex actions gouvern the process of breastfeeding initiation: the child's

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[1]J.P. Habicht, J. DaVanzo, and W.P. Butz, 'Mother's milk and sewage: their interactive effects on infant mortality', **Pediatrics, 18** (1988), pp. 456-461

[2]P. Lantz, M. Partin, and A. Palloni, 'Using retrospective surveys for estimating the effects of breastfeeding and childspacing on infant and child mortality', **Population Studies, 46** (1992), pp. 121-139

[3]B. Holland 'Breastfeeding and infant mortality: a hazards model analysis of the case of Malaysia', **Social Biology, 34** (1987), pp. 78-93; M. Montgomery, T. Richards and H.I. Braum, 'Child health, breastfeeding and survival in Malaysia: a random effect logit approach', **Journal of the American Statistical Association, 81** (1986), pp. 297-311; S. Milman and E.C. Cooksie, 'Birth weight and the effect of birth spacing and breastfeeding on infant mortality', **Studies in Family Planning, 18** (1987), pp. 202-213. M. VanLandingham, J. Trussell and L. Grummer-Strawn, 'Contraceptive and health benefits of breastfeeding: a review of the recent evidence', **International Family Planning Perspectives, 17** (1991), pp. 131-136.

suckling stimulates the mother's let-down reflex and the resultant milk flow encourages the child to continue. In healthy babies, the suckling reflex is strongest in the first 30 minutes after birth[4]. Delay in the time of first feeding can lead to difficulties in initiating and continuing breastfeeding. Unhealthy babies often have particularly weak suckling reflexes and are therefore likely to find breastfeeding more difficult. Alternative feedings and separation from the mother may exacerbate any initial difficulties. As a consequence, infants who have trouble breastfeeding at birth experience higher failure rates at all subsequent durations.

Reverse causation complicates the analysis of the links between breastfeeding and infant mortality. Children who are ill may cease to breastfeed either as a result of their own weakness (inability to suckle) or by their parent's choice. In the absence of prospective longitudinal data, the literature contains three approaches to the study of breastfeeding and its impact on child survival. These can be used in studying the distortions produced by reverse causation bias.

The first approach focuses on measuring the intentions of pregnant women to initiate breastfeeding and to breastfeed for a

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[4]M. Leefsma, 'The influence of hospital routine on success and breastfeeding', in S. Freir and A.I. Eidelman (eds.) **Human Milk, Its Biological and Social Value** (Excerpta Medica, 1980)

given duration[5]. The second approach focuses on the initiation and duration of breastfeeding, investigating the reasons for not initiating or for discontinuing breastfeeding. Studies of this type frequently examine the correlates of the initiation or duration of breastfeeding[6]. The third approach is to ask women directly the reasons why they did not breastfeed or why they stopped breastfeeding[7].

Each of these approaches has advantages and disadvantages that result from the limitations of the data. The Demographic and Health Survey (DHS) project makes it possible to combine elements from these three methodological approaches to study reverse causation bias in breastfeeding behaviour and child survival in

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[5]T. Baranowski, D. Bee, D. Rassin et al., 'Social support, social influence, ethnicity and the breastfeeding decision', **Social Science and Medicine**, **21** (1983), pp. 1599-1672; J. Lizarraga, J. Maehr, D. Wingard and M. Felice, 'Psychological and economic factors associated with infant feeding intentions of adolecent mothers', **Journal of Adolecent Health, 13** (1992), pp. 676-688; E. Romero-Gwynn and L. Carias, 'Breastfeeding intentions and practices among Hispanic mothers in Southern California', **Pediatrics**, **84** (1989), pp. 626-630.

[6]J. Stewart, B. Popkin, D. Guilkey et al., 'Influences on the extent of breastfeeding: a prospective study in the Philippines', **Demography**, **28** (1991), pp. 181-193; L. Adair, B. Popkin and D. Guilkey, 'The duration of breastfeeding: how is it affected by biological, socioeconomic, health sector, and food industry factors?', **Demography**, **30** (1993), pp. 63-72.

[7]B. Winikoff, M. Castle and V. Laukaran (eds.) **Feeding Infants in Four Societies: Causes and Consequences of Mother's Choices**. New York: Greenwood Press (1988); A. Wright, C. Clark and M. Bauer, 'Maternal employment and infant feeding practices among the Navajo', **Medical Anthropology Quarterly**, **7** (1993), pp. 260-271; T. Greiner, P. Esterik and M. Latham, 'The insufficient milk syndrome: an alterative explanation', **Medical Anthropology**, **5** (1981), pp.233-242.

countries of sub-Saharan Africa. Quality data from this region have been scarce, and the question of reverse causation has not been adequately addressed.

Breastfeeding has both contraceptive and health benefits. It improves child survival by decreasing an infant's exposure to contaminated food and water and by lengthening the interval between births, which reduces premature weaning and sibling competition for food, and reduces nutritional depletion of the mother[8]. These potential benefits are greatest in developing countries with low contraceptive prevalence, little supplementation of breast milk, low nutritional status of mothers, and cultural taboos against sexual relations for the duration of breastfeeding[9].

Holland[10] attempted to minimize the problem of reverse causation bias for infants in age groups beyond 1-2 months by conditioning the analysis on breastfeeding behavior prior to the start of each age interval. This approach, however, cannot be

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[8]J.P. Habicht, J. DaVanzo and W.P. Butz, 'Does breastfeeding really save lives or are apparent benefits due to biases?, **American Journal of Epidemiology, 123** (1986), pp. 279-290.

[9]S. Huffman and B. Lamphere, 'Breastfeeding performance and child survival', in W. H. Mosley and L. Chen (eds.) **Child Survival: Strategies for Research**, a Supplement to Volume 10 **Population and Development Review** (Cambridge: Cambridge University Press, 1984), pp. 93-116; A. Palloni and S. Millman, 'Effects of inter-birth intervals and breastfeeding on infant and early childhood mortality', **Population Studies, 40** (1986), pp. 215-236.

[10]loc. cit. in fn. 3

used in the first age interval. Children who have never been breastfed will exhibit relatively high mortality, because some of them will have been too sick at birth to initiate suckling. In principle, this bias could be reduced by asking women why they never breastfed their infants. Holland did remove those infants who were not breastfed because "the child died." However, it seems likely that the response "too sick to suckle" may sometimes be a rationalization, especially if the child died; if so, removing such infants would result in a bias in the other direction.

Other research suggests that the problem of reverse causation does not substantially exaggerate the strong effect of breastfeeding on child health. Millman and Cooksey[11], using data from the same Malaysian Family Life Survey (MFLS) as Holland, found that controlling for a number of proxy measures for the child's health status at birth had little influence on the effect of breastfeeding on infant survival. Using a different approach, Montgomery et al.[12] tested the same MFLS data for selection bias and concluded that "the direct influence of breastfeeding on survival remains of overwhelming importance even after corrections for selection bias are made."

Perhaps the best form of data available for addressing the

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[11] loc. cit. in fn. 3

[12] loc. cit. in fn. 3

reverse causation issue is an event history. Forste[13] analyzed the causal ambiguity between breastfeeding and death using an event-history technique applied to a Gompertz function. Mortality, the dependent variable, was analyzed in two ways. First, infants who died during the first month of life or were less than one month old at the time of the interview were included as having survived one month. In the second approach, only those infants who had survived at least one month of life at the time of the interview were included. The logic of this procedure is that the causal direction between breastfeeding and death is ambiguous in the neonatal cases included in the first approach. However, because breastfeeding is begun shortly after birth, the second approach contains only cases in which death is known to have followed the beginning of lactation. Comparisons between the two approaches indicate the degree of breastfeeding bias that arises during the first month of life. Forste's conclusion is that "illness is a dominant factor influencing mortality" in the neonatal period and that the "inclusion of neonates can exaggerate the effect of breastfeeding on mortality".

In the analysis presented here, the causal ambiguity is tested using several different models; to account for the fact that previous findings may be sensitive to the choice of model, two estimation equation approaches were explored.

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[13]loc. cit. in fn.3

2 DATA AND METHODS

This paper addresses the influence of reverse causation bias on the estimated effects of breastfeeding behaviour on infant survival in Nigeria. The data used here are drawn from the 1990 Nigerian Demographic and Health Survey (DHS-11)[14] and the Enugu Health-Seeking Behaviour Survey (1991). The DHS program is an ongoing worldwide collaborative effort between the governments of the survey countries and the Institute for Resource Development (Macro International, Calverton, Maryland) with financial support from the United States Government. The Nigerian DHS is a nationally representative sample of 8,781 women aged 15-49 years. The information collected includes: demographic characteristics of the survey respondent, a reproductive history, knowledge and use of contraception, fertility preferences, maternal care during pregnancy and delivery, breastfeeding, vaccination status of children, prevalence and treatment of diarrhea, fever, and cough in children, and height and weight of children.

A smaller sample consisting of 2,011 Igbo women who had live births during the five years prior to the survey date was employed as a supplement to the Nigerian DHS. The Enugu study, which conducted a methodologically similar survey as the DHS, is more specific to the Igbo ethnic group in eastern Nigeria and contains more detailed morbidity information than DHS. The

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[14]Federal Office of Statistics & Macro International Inc., **Nigerian Demographic and Health Survey 1990**, (Calverton, Maryland: FOS and MI, 1992).

instrument was designed specifically to measure health-seeking behavior among Igbo women who had live births during the five years before the survey. An advantage of this study is that, unlike the DHS, the Enugu survey collected information on both dead and surviving children, which will be helpful in investigating reverse causation. It can therefore serve to confirm findings from the larger national survey.

The sampling characteristics of the DHS provide generalizable results and permit the examination of the subgroups on which research interest is focused. In addition, the nature of the fertility history makes it possible to specify the correct timing of most explanatory variables. This is because DHS coded age at death in completed months, thus removing any restrictions on estimated hazard models. In addition, the DHS provides much more specific information on prenatal and postnatal health practices, breastfeeding, and anthropometric indicators than were available in previous studies.

2.1 Outcome variable

The outcome variable of interest is the survival status of children aged 0-12 months at the time of the survey. For each birth, the child's survival status at the time of the survey was obtained. If deceased, the child's age at death was recorded: in days if less than one month, in months if less than two years, or in years. If alive, the child's age at last birthday was obtained. Date of birth, survival status, and age at death were used to identify for each interval (0-12 months of age): (1) births exposed to the risk of death in the entire interval; and (2) among these births, those who experienced death in the interval. Although the parameters of the breastfeeding distribution can be estimated from data on the last live birth, the next to last live birth, or births in the five years preceding the survey, the distributions derived from these intervals tend to be biased[15]. The method for describing the distribution of breastfeeding employed here uses data on current breastfeeding status for the last child who breastfed.

2.2 Explanatory variables

Both the DHS and the Enugu survey collected information on many variables that are useful for investigating child survival and breastfeeding. Of the five categories of proximate mortality determinants identified by Mosley and Chen[16], for example, variables representing four were included in DHS: maternal factors (age, parity, and birth spacing), environmental factors (source of drinking water and material of the dwelling units), nutritional status (breastfeeding practices and anthropometric measurements), and personal illness control (circumstances

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[15]H.J. Page, R.J. Lesthaeghe and I.H. Shah, 'Illustrative analysis: Breast-Feeding in Pakistan', **Scientific Report No. 37**, (ISI/WFS, Voorburg: The Netherlands, 1982).

[16]W.H. Mosley and L. Chen, 'An analytical framework for the study of child survival in developing countries', **Population and Development Review**, Supplement to Volume **10** (1984), pp. 25-45.

surrounding childbirth, child immunizations, and illness treatment).

Breastfeeding information from both the DHS and the Enugu survey can be analyzed in two ways. It could be ascertained whether a woman was still breastfeeding her most recent child at the time of the survey. On the assumption that women do not breastfeed one child after the birth of the next, current breastfeeding status information (yes/no) can be used to determine the proportion of children who are still breastfed by single months of age at interview. Various authors have drawn different conclusions about current status data[17]. Careful consideration of sampling frames, including a key distinction between samples of children and samples of women, shows that current status measures are unbiased estimates of the survival function for samples of births that occur within a fixed period[18]. This is easier to determine with DHS data because they are coded in completed months rather than age segments. On the other hand, retrospectively reported ages of weaning for children who are no longer breastfeeding can be used. As Lesthaeghe and

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[17]A.M. John, J.A. Menken and J. Trussell, 'Estimating the distribution of interval length: current-status and retrospective history data', **Population Studies**, **42** (1988), pp. 115-127; R.J. Lesthaeghe and H.J. Page, 'The post-partum non susceptible period development and application of model schedules', **Population Studies**, **34** (1980), pp. 143-169.

[18]J. Trussell, L. Grummer-Strawn, G. Rodriguez and M. VanLanddingham, 'Trends and differentials in breastfeeding behavior: evidence from the WFS and DHS', **Population Studies**, **46** (1992), pp. 285-307.

Page[19] have shown, retrospective reports commonly display marked heaping at durations 6, 12, and 18 months. This heaping may be genuine, reflecting norms about appropriate weaning times. More importantly, these authors' own comparisons of current status survival curves with analogous curves computed by life-table techniques from data on age at weaning show clearly that such heaping is less evident in current status measures[20].

In the DHS, information on duration of weaning is collected for all births in the five years preceding the survey. Including all births that occurred during a fixed time period preceding the survey avoids biasing results upwards by excluding births from women with short birth intervals when the reference period is short. In our analysis, care was taken to include only those children for whom accurate breastfeeding information was available.

Prenatal and postnatal indicators relevant to healthy birth outcomes include the timing of prenatal health care, birthweight, and breastfeeding. Previous research suggests that timely prenatal care and the changes in health practices it may induce are important for healthy birth outcomes[21]. In fact, for those

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[19]loc. cit. in fn. 15

[20]They are subject to larger sampling variability than are measures based on retrospective data for a sample of the same size.

[21]S. Singh, A. Torres and J.D. Forrest, 'The need for prenatal care in the United States: evidence from the 1985 national natality survey', **Family Planning Perspective, 17**

women most likely to experience high-risk pregnancies, the quantity and quality of care may be critical in promoting healthy birth outcomes[22]. High-risk mothers include those who are poorly educated, unmarried, or young at the time of conception. Complications in pregnancy are more likely to occur for women who receive late prenatal care (the third trimester of pregnancy) or no prenatal care at all. Prenatal care is associated with higher birthweight[23], which in turn is related to a lower risk of infant death. Two causes can generally be hypothesized as contributing to unequal prenatal care in developing countries: inadequacies of the health care system and the corresponding urban bias. Gortmaker found that women with disadvantaged backgrounds (like rural or illiteracy) may be less well integrated into society, as indicated by their sparse interactions with social organizations, institutions, and agencies. Thus, they may possess fewer skills and less knowledge in seeking prenatal care. The probability of a child's mother practicing healthy behavior while pregnant therefore seems likely to be affected by many socioeconomic and demographic factors, such as the economic status of the

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(1985), pp. 118-124.

[22]J. DaVanzo, J.P. Habicht and W.P. Butz, 'Assessing socioeconomic correlates of birthweight in Peninsular Malaysia: ethnic differences and changes over time', **Social Science and Medicine**, **18** (1984), pp. 387-404.

[23]S. Gortmaker, 'The effects of prenatal care upon the health of the newborn', **American Journal of Public Health, 69** (1979), pp. 653-660.

household, maternal educational attainment, the birth order of the child, and the age of the mother at the child's birth. A dichotomous variable indicating whether prenatal care was obtained in the first trimester was created to measure the direct effect of breastfeeding on health outcome.

Anthropometry is the most useful practical tool for assessing the nutritional status of children. Almost any illness will impair the growth of a child, but in practice growth deficits in developing countries are caused primarily by two factors, inadequate food and infections. Infections generally influence body size and growth through their effects on metabolism and nutrition. Two useful anthropometric mesures obtainable from DHS data are height-for-age and weight-for-height. In their absence, weight and height can adequately proxy the effect of nutritional status. Several studies have shown that the ratio of weight to height provides a better indication of nutritional status than either weight or height separately[24]. The ratio of weight to height distinguishes the child who has experienced wasting. The ratio of weight to height and birthweight can therefore capture nutritional deficiencies in the absence of the standardized measures of weight-for-height and height-for-age.

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[24]R.T. Benn, 'Some mathematical properties of weight for height indices used in measures of adiposity', **British Journal of Preventive and Social Medicine, 25** (1971), pp. 42-50; A. Keys, F. Fidanza, M. Karvonen et al., 'Indices of relative weight and obesity', **Journal of Chronic Diseases, 25** (1972), pp. 329-343.

Birthweight reflects intrauterine pathology, prematurity, prematurity, and ability to survive illnesses immediately after birth[25]. It is also related to size later in infancy and hence to the ability to survive infectious illnesses in post-neonatal infancy. Low birthweight (less than 2,500g) was indicated in the analysis by a dichotomous variable. Because the health consequences of low birthweight draw attention to the importance of its associations with birth interval, birth order and the age of the mother at birth of the child, these latter variables are also included in the analysis. For example, selective breastfeeding behaviour is likely to interact with the mother's age at birth of the child, parity and birth interval. The child's birth order is also expected to affect his or her health status through its impact on breastfeeding and hence nutrition. This occurs because higher birth order children can suffer from increased strain on family resources, particularly the time available for child care.

An examination of diet and feeding practices must address three issues: (1) whether the child received breast milk for some interval of time; (2) whether it received any other form of milk in the interval; and (3) whether the child has begun to receive solid or semisolid foods in the specified interval of time. The variables used here include duration of unsupplemented and

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[25]R.S. Gibson, **Principles of Nutritional Assessment** (New York: Oxford University Press, 1990).

supplemented breastfeeding and type of supplemental or weaning foods. DHS asked about early initiation of suckling; timely first suckling (defined as suckling within 1 hour of birth) was measured by the variable colostrum. For both Nigeria and Enugu, the answers to the question "What is the reason for never or for stopping breastfeeding?" are useful for examining reverse causation.

The extent of breast milk supplementation or substitution influences the likelihood of ingestion of pathogens, which also depends on the quality of water and sanitation. The analysis here includes the interaction of the breastfeeding variables with water and household sanitation variables.

Individual skills, health, and time are the three basic elements that determine the productivity of household members. Measures of parents' time and health status are usually not available in DHS-type surveys, but skills are typically measured by the level of education attained by parents. Maternal health and nutritional status affect the health and survival of the child through the biological links between the mother and infant during pregnancy and lactation. A mother's responsibility for her own care during pregnancy and the care of her child through the child's most vulnerable life stages have direct relevance for child survival. Mother's level of education can, therefore, affect the child's survival status by influencing her attitudes and choices on prenatal medical visits, breastfeeding, food preparation, washing clothes, bathing the baby, and sickness care, and by increasing her skills in health care practices related to nutrition, hygiene, preventive care, and disease treatment. The various measures of breastfeeding behaviour and mortality and their definitions are summarized in Table 1.

Table 1 about here

3 MODEL BUILDING AND ESTIMATION

Although the central focus of this study is to examine the link between reverse causation and breastfeeding behavior on health outcome of the child, the causal mechanism is embedded in a complex causal framework that includes proximate as well as background variables. Problems of model specification can arise in assessing the effects of breastfeeding on infant mortality. Because breastfeeding is important for nutritional, immunity, and hygienic purposes (in an infectious environment), controlling for associated factors like birth order, birth interval, and mother's age the birth of her child is important.

Parity and mother's age at the (index) child's birth are potential sources of distortions because both are related to the length of a given birth interval and simultaneously exert effects on the risk of mortality[26]. In addition, both variables influence

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[26]K. Lee, R.M. Ferguson, M. Corpuz and L.M. Gartner, 'Maternal age and incidence of low birth weight at term: a population study', **American Journal of Obstetrics and Gynecology,**

the existence and duration of breastfeeding, thus creating the potential for artifactual effects between the latter and the risk of mortality[27]. A second potential problem arises from inappropriate representation of the underlying causal mechanisms reflecting the effect of breastfeeding on infant mortality. In some cases, breastfeeding never starts because conditions in utero precipitate death immediately after birth. Fetal growth deficiencies and immaturity produce births subject to a high risk of mortality even before breastfeeding begins. In these cases, death leads to no breastfeeding rather than the reverse[28]. All of these problems can lead to model misspecification and simultaneity bias that, if uncorrected, may exaggerate the positive effects of breastfeeding on child survival.

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158 (1988), pp. 84-89.

[27]A. Palloni and M. Tienda, 'The effects of breastfeeding and pace of childbearing on mortality of early ages', **Demography**, **23** (1986), pp. 31-43; J. Hobcraft, J.W. McDonald and S.O. Rutstein, 'Child-spacing effects on infant and early child mortality', **Population Index**, **49** (1983), pp. 585-618.

[28]DHS-type data, unlike the World Fertility Survey (WFS), have several advantages for measuring reverse causation. For example, in the WFS survey the ages at death were pre-coded into relatively broad categories, while in DHS ages at death were given in completed months. In the WFS there was a complete absence of any measure of health condition while the child was breastfeeding. DHS on the other hand, asked "Why did breastfeeding stop?" This question is a useful control for determining the direction of causality between breastfeeding and morbidity (and mortality) in the youngest age segments, particularly during the first month of life. The most suitable control for health conditions at birth is probably the colostrum variable, which indicates whether the child was put to the breast soon after delivery.

The effect of breastfeeding on infant and child mortality poses censoring problems that can bias the estimates and tests. This happens because the length of time a child breastfeeds is subject to the two competing risks of weaning and death. The average duration of breastfeeding among those who die before a specified age is generally shorter than among those who survive beyond that age, because death truncates breastfeeding. This produces an artificial association between breastfeeding duration and mortality. This bias can be assessed by comparing the survival of children beyond a specified age with whether or not they breastfed up to that age.

3.1 Random effects logit models

The causal ambiguity is tested first on the data using a random effects logit technique with several different models. The random effect models approach is useful in accounting for correlation, overdispersion, and subject-specific inference[29]. Over the first 12 months of life, the risk of dying usually exhibits a striking monotonic decline and endogenous causes of death are supplanted by exogenous ones[30]. It is therefore

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[29]A.Y.C. Kuk, 'Asymptotically unbiased estimation in generalized linear models with random effects', **Journal of the Royal Statistical Society Series B57**, (1995), pp. 395-407.

[30]J. Trussell and R. Hammerslough, 'A hazard model analysis of the covariates of infant and child mortality in Sri-Lanka', **Demography**, **20** (1983), pp. 1-26.

appropriate to study breastfeeding behaviour in the first 12 months of life. Following the example of Montgomery et al.[31], we used the random effects logit techniques to describe a child's state at time t months of age by a pair of zero-one random variables (B_t, S_t), where B_t equals 1 if a child is breastfed at time t, 0 otherwise, and S_t equals 1 if the child is still alive at time t, 0 otherwise. The structural relations of interest are those linking a given breastfeeding sequence $\{B_0, B_1, \ldots, B_t\}$ to the subsequent survival $\{S_{t+1}, \ldots, S_{12}\}$. Models using the Nigerian data measure lactation for three different time periods, namely age in days but less than one month at death (Model 1), 0-12 months (Model 2), and 1-12 months (Model 3), respectively.

It is well known in the demographic and medical professions that the prognosis for survival of low birthweight babies is much worse than that for normal birthweight babies[32]. Birthweight carries information not only on the child's own health and hence on the prognosis for successful breastfeeding and survival, but also on factors that are specific to the family[33]. Shared family background may, to a large extent, be due to common uterine environment and to common practices of the mother or family

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[31] loc. cit. fn. 3

[32] R.T. Benn loc. cit. fn. 23

[33] loc. cit. fn. 21

during pregnancy, in which nutrition is of key importance[34].

Simultaneity problems are avoided by taking mortality within each age segment as a function of conditions prevailing at the onset of the segment. Thus, to be included in the analysis for the age interval 1-12 months, the infant must have survived through the first month of life. These divisions will also suppress the simultaneity bias affecting the relation between following birth interval and the risk of death of the (index) child[35]. First births are treated separately from other births because they may be subjected to somewhat higher than average mortality risks[36]. The problem of censoring is also reduced when the breastfeeding variables are grouped into age segments.

Analyzing these models involves characterizing the dependence of death of children on explanatory variables and describing the associations between the response and explanatory variables. The response is health status of the child (with dead = 1 and surviving = 0). Random effects logit models define the logarithm of the odds of dying in various age segments as a function of the explanatory variables, allowing us to estimate the effects of these variables. Let Y_{ijk} be the binary indicator of the health outcome for breastfeeding child k in household j in

[34] A. Leichting, J.P. Delgado, R.E. Klein, C. Yarbrough and R. Matorell, 'Effects of food supplementation during pregnancy on birthweight', **Pediatrics**, **56** (1975), pp. 508-512.

[35] A. Palloni and M. Tienda, loc. cit. fn. 27

[36] Hobcraft et al. loc cit. fn. 26

cluster i; then a random effects logit model can be written as:

$$\text{logit } Pr(Y_{ijk}=1 \mid \epsilon_{ij}) = \beta_0 + \beta_1 S_{ij} + \beta_2 M_{ij} + \beta_3 G_{ij} + \beta_4 D_{ij} + \beta_5 P_{ij} + \epsilon_{ij},$$

where S is the vector of variables representing selection factors, M the vector of background factors, G the vector of pregnancy factors, D the vector of variables representing infant details, and P the vector of postnatal factors. The unobservable random effect ϵ_{ij} is assumed to be common to the jth household in the ith cluster. The correlation between ϵ_{ij} and ϵ_{ik} $(k \neq j)$ and the variance of ϵ_{ij} determine the design effects for each sample. The significance of an explanatory variable was tested by evaluating the change in the likelihood ratio statistic resulting from including the explanatory variable in the model[37].

Because results on the estimation of parameters may be sensitive to the choice of model, an additional estimation approach that is time-dependent was employed to examine the issue of reverse causation. The event history approach provides additional information on the timing and progress of the health condition of a sick child.

3.2 Event history approach

Many structurally similar empirical questions focus on

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[37]A. Agresti, **Categorical Data Analysis,** (New York: John Wiley & Sons, 1990)

determining how the time spent between two successive events is related to certain explanatory variables. Breastfeeding duration, in this situation, is best handled as a time-varying covariate. Censored individuals are those children for whom one does not have ending times because they survived beyond age 12 months. Censored data and time-varying covariates can be dealt with through the use of proportional hazard models, which take as their dependent variables the rates at which children move from one state to another, rather than the durations spent in the states. Each child contributes one observation for each month from the start of breastfeeding until the event of interest (time elapsed from birth to either death or censoring at age 12 months or the survey month). Each observation consists of a binary dependent variable (whether or not breastfeeding stopped in that month) and a set of explanatory variable values for that month. The unit of analysis is the person-months lived by the child, and the event studied is the death of the child during the first 12 months of life.

For both data sets, lactation is measured as a time-varying covariate and indicates whether breastfeeding never started, or stopped during the interval from age 0 to 12 months. The more general form of the proportional hazards model[38] is used in this

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[38]D.R Cox, 'Regression models and life tables (with discussion)', **Journal of the Royal Statistical Society B74**, (1972), pp. 187-220; J.D. Kalbfleisch and B.L. Prentice, **The Statistical Analysis of Failure Time Data** (John Wiley & Sons: New York, 1980).

specification. The model's underlying assumption is that heterogeneity is captured by the set of covariates considered for analysis, and relative risks remain constant over the duration of breastfeeding. The model is of the form:

$$\mathbf{h}_{(t,z)} = \mathbf{h}_{0(t)} \; \mathbf{exp}(\mathbf{ß Z})$$

where $h_{(t,z)}$ is the time elapsed from birth to either death or censoring, at age 12 months or the survey month, for a child with a vector of covariates **Z**; **ß** is a vector of unknown regression coefficients to be estimated; and $h_{0(t)}$ is an arbitrary nonnegative, unspecified baseline hazard function not dependent on the covariates. The hazard function enables one to estimate the relative risks in terms of the baseline group by exponentiating the regression coefficients. Each exponential of the coefficients in the equation above represents the effect of the covariate on the hazard function for the reference category. Any value greater than 1 indicates a relative risk of death greater for this category than for the reference category, while a value less than 1 indicates a smaller relative risk of death.

Some analysts find the exponentiated parameter estimates easier to understand than the parameter estimates themselves. These exponentiated estimates can be interpreted directly as odds of death. However, the approach below is to present in tabular form only the parameter estimates themselves for the logit models. These will often be converted to odds ratios in the discussions. The results of the hazard models are, on the other hand, presented as hazard ratios.

3.3 Survival curves

We plotted the survival curves for seven groups of breastfeeding infants, using the Stata procedure "Kapmeier"[39]: (1) weaned, (2) mother ill, (3) insufficient milk, (4) mother pregnant, (5) child ill, (6) mother working, and (7) still breastfeeding. The formula for these survival curves is:

$$S_t = \Pi_{j<t}(n_j - d_j)/n_j,$$

where d_j is the number of deaths at time j among breastfeeding children and n_j is the number of observations at risk of death. To obtain d_j and n_j, we let k = 1,....,N index the individual observations. Define n_{kj} to be 1 if child k is alive at time j and 0 otherwise, and d_{kj} to be 1 if child k died at time j and 0 otherwise. Then $d_j = \Sigma_k d_{kj}$ and $n_j = \Sigma_k n_{kj}$. The procedure merely counts the alive observations and the number dying at each specified time.

4 RESULTS

Figure 1 shows the Kaplan-Meier survival curves for the seven breastfeeding groups defined above. This product-limit estimate of the survival curve produces a descending step

[39]StataCor, **Stata Statistical Software Release 4.0** (College Station, TX: Stata Corporation, 1995)

function where each drop indicates the number of deaths. Of particular interest is the curve for Group (5), children who were reported ill at the time of the survey. It reflects the survival status of those children who were ill in the early months and died over the course of the first 12 months. The fact that this curve drops faster than any other group's curve shows that illness is associated with hampered breastfeeding behaviour in children. The graph shows that most of the children in Group (5) died before their first birthdays, and all of those who survived the first birthday did not reach their second birthdays.

Figure 1 about here

The Group (4) category, pregnancy, is an important reason for cessation of breastfeeding between 6 and 12 months of life; it is the closest to Group (5) in steepness. The other curves do not appear to fall steeply, which suggests that breastfeeding was not interrupted much for these categories. The graph shows on the whole that not only do the influences of the grouping criteria on breastfeeding practices vary with age of the child, but they also vary with the reasons reported for initiating or stopping breastfeeding within a given age interval.

We compared the survival curves of the various groups by computing the logrank statistics under the assumption that the underlying survival curves are the same, using the χ^2

statistic[40]. The results indicate that the groups are indeed different in both their reason-specific and age-specific breastfeeding behaviour ($p < 0.01$ in each case). For the control group (still breastfeeding), there were 16 observed deaths, but only 3.68 would be expected if all the children had the same survival rate. This indicates that breastfeeding for a longer period (here, 6 or more months) has a beneficial effect on infant survival.

We now compare the estimated logit models in Table 2 for Nigeria and Table 3 for Enugu to gain more insight into the breastfeeding bias attributable to illness. Because it is difficult to discern the causal relationship between breastfeeding and illness or death during the first month of life, the models were constructed to reflect the effect of the inclusion of the neonates. Children who experience health complications immediately after birth may never begin breastfeeding at all. If such children experience recurrent illness or die early, an association between never having breastfed and failure to survive will be observed, regardless of the actual effects of breastfeeding. Removing this bias requires controlling for the health status of the child at birth and shortly thereafter. The rationale for omitting the age category 0 months in the subsequent model is that those children who

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[40]R. Peto and M.C. Pike, 'Conservatism of the approximation of the chi-square in the logrank test for survival data or tumor incidence data', **Biometrics**, **29** (1973), pp. 579-584.

survived the first month of life were less likely to experience conditions that would simultaneously affect their health and their mothers' breastfeeding behaviour. Model 1 (Table 2) for the Nigerian data clearly includes all children who died before reaching the age of one month. With the benefit of the hindsight, one expects that effects estimated with and without controlling for health conditions at birth are roughly of the same order of magnitude as those estimated without any controls. Model 2 includes all children aged 0-12 months while Model 3 excludes children aged 0 months for the Nigerian data. In the case of Enugu, Model 1 (Table 3) includes all children aged 0-12 months while model 2 includes children aged 1-12 months only.

Table 2 about here

4.1 Reverse causation and breastfeeding

An examination of the coefficients for lactation in Model 1 (Table 2) for Nigeria shows that the inclusion of younger children increases the size of the effects of the illness variable greatly. For Nigeria, the odds of death increase by a factor of 2.1 (=exp(0.719)) from the baseline value when a breastfeeding child aged < 1 month is ill ($p<0.01$). From Table 2, the odds of death for breastfeeding children who become ill increase by a slightly smaller factor of 1.9 (=exp(0.638), $p<0.01$) when the age at death is widened from 0 months to 0-12 months. The increase in the odds of death is further reduced to a factor of 1.4 (=exp(0.336), $p<0.05$) when children aged 0 months are excluded from the equation. For Nigeria, the level of significance decreases with the exclusion of the young infants (aged less than one month). For Enugu, Model 1 (Table 3) shows that the odds of death for children aged 0-12 months increase by a factor of 26.7 ($p<0.01$) for children who are too ill to breastfeed. Table 3 further shows for Enugu that the odds of death to breastfeeding children who become ill increase by a dramatically smaller factor of 5.1 when the age at death does not include deaths in the neonatal period. It appears that changing breastfeeding practices as a result of illness can increase the child's susceptibility to death more in the younger age segment.

Table 3 about here

The sizes of the coefficients strongly suggest that the problem of reverse causation is strongest during the neonatal period.

Based on the results of Tables 2 and 3, prior illness does appear to influence the effect of breastfeeding on mortality, particularly in the neonatal segment. To address the question of the magnitude of this influence, the event history approach was applied to the Nigerian and Enugu data sets in Tables 4 and 5. The problem of reverse causation is less amenable to solution than the issue of prevailing health condition of the child. A child who is ill may cease to breastfeed either as a result of his/her physical condition (making it difficult to suckle) or as a result of parental decision based on the child's health condition. If the child dies from the illness or its consequences, an observer who lacks information on the timing and progress of the illness could conclude that cessation of breastfeeding explains the death. An obvious but difficult solution is to collect observations not only on the survival status of the children but also on their illnesses. If such information is not available, the best one can do is to use alternative solutions that minimize the impact of the biases[41]. Because this problem is the least tractable with retrospective information, the effects can be studied better with a time-varying covariate. Therefore, models in Tables 4 and 5 provide additional information on reverse causation.

The analysis of results presented in Table 4 for Enugu was done in two steps. First, a measure of breastfeeding that disregards the information on illness (or the reasons for its discontinuation) was included. This measure is a time-varying covariate that indicates whether breastfeeding never started, or when it stopped. Second, a dummy variable indicating when illness of the child caused breastfeeding to stop or never to start was included in the model with the control variables. The assumption here is that if breastfeeding ceases because of illness and if illness is the primary factor leading to death, then these two measures can be assumed to separate the effect of illness,

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[41]loc. cit. fn. 8

operating through breastfeeding, from the direct effect of lactation on mortality. In Table 4, Model 1 estimates the lactation effects without the illness variable; Model 2 includes the illness variable without the controls for sanitation; and Model 3 includes both the illness variable and the sanitation variables.

Tables 4 about here

Results of Model 1 show that infants who stopped breastfeeding were 2.3 times more likely to die before their first birthday than infants who continued to breastfeed, although the hazard ratio is not significantly different from zero ($p>0.10$). In Model 2, the effect of the inclusion of the dummy variable indicating illness is to reduce the hazard ratio of breastfeeding cessation from 2.3 to 1.9, and to produce a highly significant effect on mortality (the hazard ratio for the illness variable being 3.6 times with $p<0.01$). This is a clear indication that prior illness is a primary factor leading to death, and therefore, its inclusion in a model with breastfeeding measures can overestimate the effect of lactation on mortality, at least in the early months of the child's life.

Unlike Enugu, two mortality models were examined with the Nigerian data in order to discern the causal relationship between breastfeeding and illness or death (Table 5). In Model 1, infants who died during the first month of life or those who were less than one month old at the time of the interview were included in the sample as having survived one month. Model 2, on the other hand, included only those infants who survived at least one month, so that the dependent variable includes death to births aged 1-12 months. In these cases, the interval from the index birth to succeeding conception is time-dependent. Lactation is similarly measured as a time-dependent covariate.

Table 5 about here

Examination of the hazard ratios in Table 5 shows that the model with the neonatal cases is highly significant for the illness variable (the hazard ratio being 1.53, $p< 0.01$). For the model without the neonates, the hazard ratio is 1.22 ($p<0.05$). We also find that if breastfeeding stopped for other reasons, child survival improves more for the age segment 1-12 months than it does for the age segment including the neonatal births (the hazard ratios being 0.58 and 0.83 respectively), although the evidence in both cases is weak.

Table 5 also shows that low birthweight babies contribute to higher levels of mortality in the age segment 0-12 months than in the age segment 1-12 months (the hazard ratios being 1.32, $p<0.01$; and 1.09, $p<0.05$). Although children not fed colostrum soon after delivery have a higher hazard ratio than those in the reference category, the effect is not statistically significant. An increase of one unit in the weight/height ratio increases the odds of death by a factor of 1.75 ($p<0.01$) for the model including the neonatal deaths, while for the model excluding neonates, the odds of death are increased by a factor of 1.38 ($p<0.05$). The difference between the hazard ratios for the two models suggests strongly that reverse causation bias is likely to affect breastfeeding infants in the neonatal stage more than those who survived this age segment. These findings are consistent with evidence that suggests a close association between health conditions of the child (and perhaps the mother) at birth and the probability of initiating and continuing breastfeeding.

On the whole, higher education, no previous child death, birth intervals greater than 18 months, and birth order 2-3 contribute significantly to reducing infant mortality relative to the reference categories. On the other hand, children whose mothers did not seek any prenatal service or did not have tetanus vaccination during pregnancy, as well as short birth interval babies, are significantly affected by mortality in comparison to the reference categories.

5 SUMMARY AND CONCLUSIONS

The risk of mortality is much higher for breastfeeding children who were ill during the neonatal segment than it is for normal breastfeeding children at this age. It is often concluded that the lack of breastfeeding could be the cause of the elevated mortality. This problem arises because it is not known whether the decision never to breastfeed or to stop breastfeeding is made by choice or necessitated by illness of the mother or the child. The information necessary for analyzing this problem was available for both the Nigerian DHS and the Enugu survey ("What is the reason for never or for stopping breastfeeding?"). The results presented above show that for children too ill to breastfeed, the cause of not breastfeeding is the illness. The analysis shows that low birthweight babies are more likely to discontinue breastfeeding or not to initiate it at all in the early days after birth.

The question of how important the confounding effects of reverse causation are to the survival of a breastfeeding child would likely have negative implications for low birthweight and sick babies than to normal birthweight babies. This observation also supports the immunological and biochemical evidence that the benefits of breastfeeding are likely to be greatest early in the first year of life, when the infant's enzymatic systems are not fully developed and its defenses against disease are weak.

In traditional societies such as Nigeria, it is hard to believe that a mother would withhold breast milk from the child in the event that the child is ill. Mothers who did not breastfeed their children were rather rare in the samples (about 6% in both cases) and the proportion of infant deaths among these children was minute. Thus, given the fact that breastfeeding usually begins soon after delivery, illness, in our view, is the major factor in preventing the breastfeeding of children.

Table 1. Definition of variables used to address the reverse causation issue in the mortality models

Variables	Operational definition	Comments
Outcome		
Mortality (births 0 months)	Period in days, weeks, or less than one month when death occurred	Assumed to have survived one month
Mortality (births 0-12 months)	Period in months from birth to death; right-censored at interview data or age 12 months	To account for neonatal mortality
Mortality (births 1-12 months)	Period in months from age one month after birth to first birthday; cases are right-censored at date of interview or age 12 months	Infants who died in the first month of life were excluded because breastfeeding may have begun after death
Explanatory		
BF stopped because of illness	A time-dependent covariate accounting whether BF started or stopped or never for reasons of illness only	To show that illness determined BF behavior
BF stopped for other reasons	A time-dependent covariate to account for other reasons why BF stopped	Mainly to exclude the illness variable
Controls		
Age of mother at child's birth	Child's birthdate in century months minus mother's DOB in century months Categories:<19; 19-29; and >29 years	To monitor selection bias due to age bounds of mother and child care practices
Colostrum	Dummy variable indicating if baby was put to breast soon after delivery	Indicates how soon breastfeeding begun
Birth order	Indicated by a set of dummy coding for births before the index child who is breastfed	Omitted category is most prevalent group
Birth interval	Indicator variables for the intervals <18 months; 18-24 months; >24 months	Monitors the role of gestational age and low bith weight babies

Background		
Education of mother	A set of dummy coding to capture: no schooling; Primary; Secondary and Higher education	A proxy for mother's knowledge of health and child care
Prenatal care		
Tetanus injection	Dummy for whether mother was given tetanus injection (=1) at pregnancy	To monitor tetanus infection, a serious infant killer
Prenatal visits	A dummy variable indicating if mother made a visit during the first trimester	Monitors fetal health untill birth
Place of delivery	A set of dummy coding to indicate where baby was delivered: Hospital/clinic; Home; Others	Home delivery is the category omitted
Infant details		
Sex of child	Dummy variable indicating sex of the index birth	Can monitor biological differences at birth

Table 2. Random effect logit models of breastfeeding and related factors on mortality of children aged 0-12 months before the survey: Nigeria (DHS) 1990

	Age at death					
	Model 1 < 1 month		**Model 2** 0-12 months		**Model 3** 1-12 months	
Covariates	Beta	SE	Beta	SE	Beta	SE
Breastfeeding stopped(still=0)						
Mother ill	0.273*	0.192	0.266	0.232	0.259	0.231
Insufficient milk	0.437*	0.354	0.422	0.388	0.420	0.385
Child ill	0.719***	0.347	0.638***	0.383	0.336**	0.161
Other reasons	0.510	0.473	0.376	0.365	0.298	0.237
Fed colostrum(no=0)	-0.383	0.291	-0.372	0.293	-0.259	0.276
Birth weight(<2500g)	-0.344	0.403	-0.320	0.397	-0.318	0.392
Weight/height	-0.599***	0.187	-0.563***	0.189	-0.524**	0.235
Age at birth(18-29=0)						
< 18 years	0.237	0.309	0.229	0.312	0.220	0.313
30+ years	-0.138*	0.088	-0.135*	0.093	-0.131*	0.095
Birth order(1st birth=0)						
2-3	-0.148**	0.107	-0.143**	0.109	-0.142**	0.108
4+	-0.277	0.185	-0.273	0.188	-0.272	0.189
Previous child died(yes=0)	-0.253**	0.098	-0.249**	0.097	-0.232	0.107
Tetanus vaccination(no=0)	-0.320*	0.261	-0.329*	0.260	-0.332**	0.198
Education(no=0):						
Primary	0.129	0.125	0.127	0.122	0.123	0.120
Higher	-0.211	0.201	-0.213	0.209	-0.217	0.209
Intercept	0.587		0.419		0.417	
No of observations	610		3276		3235	
Log likelihood	-236.0		-1020.6		-1009.8	

* = p<0.10; ** = p<0.05; *** = p<0.01

Table 3. Random effect logit models of breastfeeding status and related factors on child mortality for most recent births: Enugu 1991

Variable	Age at death			
	0-12 months		1-12 months	
	Model 1		**Model 2**	
	ßeta	SE	ßeta	SE
Why BF stopped(still BF=0):				
Insufficient milk	0.833	0.706	0.975	0.788
Mother too ill	0.390**	0.205	0.213	0.225
Child too ill	3.283***	0.517	1.627**	0.517
Weaned	-0.237**	0.101	-0.133***	0.096
Birth order (1st birth=0):				
2-3	-0.530**	0.255	-0.519**	0.250
4 or more	-0.244	0.193	-0.230	0.190
Age at birth (18-29=0)				
< 18 years	0.370**	0.183	0.473	0.221
30+ years	-0.217	0.201	-0.139*	0.195
Previous child died(yes=0)				
No death	-0.397***	0.103	-0.286	0.283
Toilet facility(pit=0)				
No facility	0.336***	0.091	0.265***	0.068
Flush toilet	-0.446***	0.129	-0.451***	0.135
Others	0.439	0.362	0.194	0.338
Water(rainwater=0)				
Private tap	-0.463*	0.269	-0.426*	0.251
Well	-0.173*	0.073	-0.156*	0.062
Other	0.576	0.424	0.371	0.537
Prenatal care(yes=0)				
No care	0.378**	0.213	0.359*	0.256
Tetanus vaccination(yes=0)				
No vaccination	0.572***	0.210	0.553**	0.323
Education(no=0)				
Primary	0.125	0.122	0.121	0.120
Higher	-0.233*	0.198	-0.232*	0.197
Intercept	-4.339		-4.294	
Number of cases	1933		1898	
Log Likelihood	-202.2		-197.3	

Note: BF = Breastfeeding
* = p < 0.10; ** = p < 0.05; *** = p < 0.01

Table 4. Estimated hazard models of breastfeeding effects and related factors on mortality of children aged 0-12 months before the survey: Enugu 1991

Covariate	Model 1[a] Hazard ratio	Model 2 Hazard ratio	Model 3 Hazard ratio
Still breastfeeding	1.0	1.0	1.0
No illness[b]	2.3	1.9	1.9
Child ill[b]	-	3.6***	3.3***
Other reasons[b]	0.8***	0.7***	0.6***
Age at birth:			
26 or more years	1.0	1.0	1.0
< 19 years	2.6**	2.5**	2.0**
19-25 years	0.8**	0.6***	0.6***
Birth order:			
1st order	1.0	1.0	1.0
2-3 order	1.7*	1.5*	1.5*
4+ order	1.6	1.3	1.2
Type of water:			
Rainwater			1.0
Private tap			0.7
Well			0.6**
Toilet faciloty:			
Pit latrine			1.0
Flush toilet			0.4
Number of cases	2082	2111	1928
Log likelihood	-3448.6	-3426.3	-3406.9

Notes: [a]This model excludes 29 observations for which children were too ill to suckle; but were finally included in Models 2 and 3.

[b] Time-varying covariate

$p < 0.10$ = *; $p < 0.05$ = **; $p < 0.01$ = ***

Table 5. Estimated hazard models for the covariates of child mortality for sample of most recent births: Nigeria DHS 1991

Variable	Age at death 0-12months	1-12 months
	Model 1 Hazard ratio	**Model 2** Hazard ratio
Why breastfeeding stopped:		
Still breastfeeding	1.00	1.00
Child ill[1]	1.53***	1.22**
Other reasons[1]	0.83	0.58
Fed colostrum		
Yes, colostrum	1.00	1.00
Waited	1.37	1.15
Fed formula before 4 months		
Yes	1.00	1.00
No	0.45**	0.42**
Fed solids before 4 months		
Yes	1.00	1.00
No	0.67**	0.63**
Birth order		
1st birth	1.00	1.00
2-3 births	0.61**	0.60**
4+ births	1.13	1.13
Birth interval		
25 months or more	1.00	1.00
<18 months	1.17**	1.15*
18-24 months	0.57	0.63
Birth weight		
>2500 grams	1.00	1.00
Less than 2500g	1.32**	1.09*
Previous child died		
Yes	1.00	1.00
No	0.73*	0.69*
Weight/height	1.75***	1.68**
Age at birth		
18-29 years	1.00	1.00
<18years	1.21*	1.02
30+ years	0.55	0.33
Tetanus vaccination:		
No vaccination	1.00	1.00
Yes vaccination	0.69*	0.54**
Number of cases	8041	8033
Log Likelihood	-1262.3	-1206.6

Note: [1] Time-dependent covariate
* = p < 0.10; ** = p < 0.05; *** = p < 0.001