Breastfeeding behavior and infant survival with emphasis on reverse causation bias: Some evidence from Nigeria

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PLEASE SCROLL DOWN FOR ARTICLE
Breastfeeding Behavior and Infant Survival
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Some Evidence from Nigeria

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ABSTRACT: The possibility of selection bias in the estimation of the effects of breastfeeding on
subsequent survival is implied by the clinical evidence that children who are healthier at birth
are more likely to be breastfed than their less healthy counterparts who may be prone to difficul-
ties in sustaining breastfeeding. This paper addresses an important problem in understanding
the association of breastfeeding and child survival with regard to reverse causation. It utilizes
data on the reported reason for weaning to assess the degree to which reverse causality may be
responsible for observed associations. The analysis indicates that children who are weaned in the
neonatal period because of illness or weakness to suckle, experience a much higher risk of dying
than others. This is not mainly because of the cessation of breastfeeding, but because of the origi-
nal factor, being their illness. Any biases imparted by an initial selection mechanism appear,
therefore, to have influence on the effectiveness of breastfeeding behavior.

INTRODUCTION

Demographic and public health communities generally agree that breastfeeding provides early protection against dis-
ease for infants in environments where the alternatives to breast milk can easily
be contaminated by poor household con-
ditions (Habicht et al., 1988). 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
(Lantz et al., 1992). Reverse causation
bias is said to occur when the infant is
too sick to suckle, so that death leads to
“weaning” rather than the reverse (Hol-
lund, 1987; Montgomery et al., 1986;
Milman and Cooksie, 1987; VanLanding-
ham et al., 1991). It arises from a child’s
inability to initiate or sustain breastfeed-
ing and can therefore be complicated by
the child’s initial health status.

The relationship between breastfeed-
ing cessation (due to ill health) and
mortality is not simultaneous, but rather
recursive; changes in breastfeeding be-
havior may not alter health outcome in-
stantly, but only after a time lag. If there
are unobserved factors influencing health
status and the type of decision made by
parents to discontinue breastfeeding, any
direct links between breastfeeding and
health outcome may be confounded by
unmeasured variables in the data. Re-
verse causation addresses the bias that re-
sults from death due to the cessation of
breastfeeding by infants who become ill.
The causal model assumes that neonatal
illness can result in the death of the child
either directly (through illness) or indi-
rectly (as a result of the cessation of
breastfeeding, which is known to impart
health benefits to the infant). Both the di-
rect effect (illness to mortality) and the
indirect effect of illness (operating via
cessation of breastfeeding) can produce high mortality. In addressing the issue of reverse causation bias, we want to determine the impact of breastfeeding cessation vis-à-vis the illness factor on the child's health status prior to death. This paper addresses the problem in understanding the association of breastfeeding cessation due to illness and child mortality. It utilizes data on the reported reason for weaning to assess the degree to which reverse causality may be responsible for observed associations.

**REVERSE CAUSALITY AND BREASTFEEDING BEHAVIOR**

Successful breastfeeding is the result of complex psychological and physical interactions between the baby and the mother, so that difficulties for either can lead to discontinuation (Jelliffe and Jelliffe, 1978). Two mutually reinforcing reflex actions govern the process of breastfeeding initiation: the child's 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 (Leefsmia, 1980). Delay in the time of first feeding can lead to difficulties in initiating and in continuing breastfeeding. Unhealthy babies often have particularly weak sucking reflexes and are therefore likely to find breastfeeding more difficult than healthy babies. Alternative feeding and separation from the mother may also exacerbate any initial difficulties. As a consequence, infants who have trouble initiating breastfeeding at birth experience higher failure rates (of breastfeeding) at all subsequent durations.

Reverse causation complicates the analysis of the links between breastfeeding and infant mortality. While it is clear that mortality is the dependent variable, the causal model (at least, its nucleus) is complex as is suggested in Figure 1. The issue of "reversed causation" is not that one really wants to know whether the strongest effect is merely the direct effect (i.e., illness to mortality) or the indirect effect (operating via cessation of breastfeeding), as is shown in Panel A of Figure 1. Rather, we want to determine the impact of breastfeeding cessation vis-à-vis the illness factor on the child's health status prior to death as presented in Panel B. Children who are ill may cease to breastfeed either as a result of their own weakness (inability to suckle) or by their parents' choice. The literature on feeding practices that include avoiding breastfeeding during illness of the child almost always produces very few cases (Naved and Kumar, 1994; Adair, Popkin and Guilkey, 1993). From Panel B in Figure 1, wasting can trigger illness or vice-versa. Weaning the child (stopping breastfeeding) can lead to wasting or illness, either one of which can lead to death. In the absence of prospective or panel data, the literature contains three approaches to the study of breastfeeding and its impact on child survival that 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 given duration (Baranowski et al., 1983; Lizarraga et al., 1992; Romero-Gwynn and Carias, 1989). 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...
FIG. 1.—Pathways of how breastfeeding can affect child survival.
(Stewart et al., 1991; Adair et al., 1993). The third approach is to ask women directly the reasons why they did not breastfeed or why they stopped breastfeeding (Winikoff et al., 1988; Wright et al., 1993; Greiner et al., 1981). 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 and child survival in developing countries. Quality data from most developing countries, in particular, countries of sub-Saharan Africa, have been scarce, and the question of reverse causation bias has not been adequately addressed.

Apart from its contraceptive benefits, breastfeeding improves child survival by decreasing the 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 also reduces nutritional depletion of the mother (Habicht et al., 1986). 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 (Huffman and Lamphere, 1984; Palloni and Milman, 1986).

Holland (1987) 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 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 (VanLandingham et al.); 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 Cooksie (1987), 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. (1986) 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 reverse causation issue is event history data. Forste (1994) analyzed the causal ambiguity between breastfeeding and survival 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 with two different data sets. To account for the fact that previous findings may be sensitive to the choice of model, two estimation equation approaches were explored: proportional hazard model using event history data and a random-effects logit model.

**DATA AND METHODS**

This paper addresses the influence of reverse causation bias on the estimated effects of breastfeeding behavior on infant survival in Nigeria. The data used here are drawn from the 1990 Nigerian Demographic and Health Survey (Federal Office of Statistics and Macro International Inc., 1992) and the Enugu Health-Seeking Behavior Survey (1991) covering a sub-region of Nigeria. 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 survey methodologically similar to the DHS, is more specific to the Igbo ethnic group in eastern Nigeria and contains as detailed morbidity information as the DHS. The instrument was designed specifically to measure health-seeking behavior among Igbo women who had live births during the five years before the survey. It can therefore serve to confirm findings at the local level that obtain in the larger national survey. The inclusion of the Enugu data is also to help in the construction of the different models employed in the analysis.

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

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*Many infants die during the first month of life before breastfeeding is begun, thus, death determines whether breastfeeding occurs and not otherwise.*
more specific information on prenatal and postnatal health practices, breastfeeding, and anthropometric indicators than were available in previous studies. In other words, the data on hand utilize information on the reported reason for weaning the child to assess the degree to which reverse causality may be responsible for observed associations.

**Outcome Variable**

The outcome variable of interest is the survival status of children in the age interval 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 as identifiers for each interval (between 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 at the end of a particular age interval between 0–12 months (defined by the analytic models presented below). Because children who die in the interval cannot possibly be exposed to the risk of death throughout the interval, their contribution to exposure is limited, and hence was dropped from the analysis. Children who died at the end of the interval were assumed to have been breastfed for a period up to the point before death.

**Explanatory Variables**

Both the DHS and the Enugu survey collected information on many variables that are useful for investigating child survival and breastfeeding behavior. Of the five categories of proximate mortality determinants identified by Mosley and Chen (1984), 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 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 status breastfeeding information (yes/no) can be used to determine the proportion of children who are still breastfed by single months of age at interview (John et al., 1988). On the other hand, retrospectively reported ages of weaning for children who are no longer breastfeeding can also be used.

There has been considerable debate about the validity and reliability of responses to survey questions regarding the duration of breastfeeding and the reasons for not initiating or for discontinuing breastfeeding (Lesthaeghe and Page, 1980). Information collected in many retrospective surveys (in many developing countries) has indicated that substantial proportions of breastfeeding durations were concentrated around digits which are multiple of three or six (heaping), and these findings have been interpreted by the analysts as evidence for defective data (Lesthaeghe and Page, 1980). Nonetheless, those data have been used by researchers because they are “objective,” unlike data on reported reasons for stopping breastfeeding, which are perceived
by some as “subjective.” While the objective data on initiation and duration of breastfeeding have provided a rationale for their use by researchers, the “subjective” breastfeeding data have been questioned with arguments that invoke their accuracy and reliability. However, detailed studies of methodological aspects of the measurement of reasons of breastfeeding cessation carried out in the 1970s (Winikoff et al., 1988; Greiner et al., 1981; Romero-Gwynn and Carias, 1989) and as part of the current DHS program (Macro International Inc., 1993) do not suggest that these data are seriously flawed.

Analyzing the MFLS-1 and MFLS-2 data in an attempt to understand the links between the “subjective” and “objective” data on breastfeeding patterns, DaVanzo and Defo (1996) concluded that there is “remarkable similarity in the distributions of reported reasons for no or short breastfeeding.” The authors found in both surveys that no/insufficient breastmilk is the most frequently given reason for no or short breastfeeding. Maternal and child health and mortality, inconvenience to mother, mother’s employment, and the child not wanting to breastfeed are the other reasons given for no/short breastfeeding in the two surveys. The authors also found a positive association between low birth weight and the likelihood of not initiating or continuing breastfeeding. Health reasons were also reported as a reason for not initiating or continuing breastfeeding among women who had no prenatal care visits. These findings are consistent with evidence that suggest a close association between health conditions of the mother and the child at birth and the probability of initiating and continuing breastfeeding. In short, the literature suggests strongly that most of the reasons given for not initiating or for discontinuing breastfeeding are not seriously flawed, and that they by and large serve their intended purposes. Data on reason-specific breastfeeding practices are, in effect, informative not only in verifying hypotheses about the influences on breastfeeding patterns generated from prospective data, but also in supplementing information on factors that covary with duration of breastfeeding that are collected by retrospective surveys like the DHS and the Enugu survey.

For the data on hand, information on duration of weaning was collected for all births in the five years preceding the survey. The inclusion of all births that occurred during a fixed time period preceding the survey avoids biasing results upward by excluding births to women with short birth intervals. In our analysis, care was taken to include only those children for whom the required breastfeeding information was available.

**OTHER EXPLANATORY VARIABLES**

Prenatal and postnatal indicators relevant to healthy birth outcomes include the timing of prenatal health care, birth weight, and breastfeeding. Previous research suggests that timely prenatal care and the changes in health practices it may induce are important for healthy birth outcomes (Singh et al., 1985). In fact, for

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3Reported reasons for stopping breastfeeding include mainly death of child, illness of mother, and insufficient milk.
those women most likely to experience high-risk pregnancies, the quantity and quality of care may be critical in promoting healthy birth outcomes (Gortmaker, 1979). 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 birth weight (Gortmaker, 1979), 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 (1979) found that women with disadvantaged backgrounds (rural and/or illiterate) 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 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 health practices on mortality.

Birth weight reflects intrauterine pathology, prematurity, and ability to survive illnesses immediately after birth (Gibson, 1990). It is also related to size later in infancy and hence to the ability to survive infectious illnesses in post-neonatal infancy. Low birth weight (less than 2,500g) was indicated in the analysis by a dichotomous variable. Because the health consequences of low birth weight 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 behavior is likely to interact with the mother's age at birth of the child, parity, and birth interval. In particular, maternal age at the time of the child's birth is expected to influence the extent of and reason-specific breastfeeding duration in at least two ways: First, a teenage mother may be less aware of appropriate breastfeeding practices and, lacking the appropriate advice, may be more likely to stop breastfeeding (if initiated at all) if she fails to produce milk after a few attempts; hence "no milk" may be given as the reason for stopping breastfeeding. Secondly, we expect that primiparas (women who have given birth the first time) should be more likely to report "no milk" as a reason for stopping breastfeeding. 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 considered here include duration of unsupplemented and 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 bias because the reported reason for weaning can now be employed in a direct analysis.

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 of 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 behavior and mortality and their definitions are summarized in Table 1.

MODEL BUILDING AND ESTIMATION

Although the central focus of this study is to examine the link between reverse causation and breastfeeding behavior on the 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 at the birth of her child is important.

Parity and mother's age at the 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 (Lee et al., 1988). In addition, both variables influence the existence and duration of breastfeeding, thus creating the potential for artifactual effects between the latter and the risk of mortality (Palloni and Tienda, 1985; Hobcraft et al., 1983). 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. 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.

The effect of breastfeeding on infant and child mortality also poses censoring problems that can bias the estimates and tests. This happens because the length of
<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>OPERATIONAL DEFINITION</th>
<th>COMMENTS</th>
</tr>
</thead>
<tbody>
<tr>
<td>Outcome</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Mortality (births 0 months)</td>
<td>Period in days, weeks, or less than one month when death occurred</td>
<td>Assumed to have survived one month</td>
</tr>
<tr>
<td>Mortality (births 0-12 months)</td>
<td>Period in months from birth to death; right-censored at interview data or age 12 months</td>
<td>To account for neonatal mortality</td>
</tr>
<tr>
<td>Mortality (births 1-12 months)</td>
<td>Period in months from age one month after birth to first birthday; cases are right-censored at date of interview or age 12 months</td>
<td>Infants who died in the first month of life were excluded because of censoring</td>
</tr>
<tr>
<td>Explanatory</td>
<td></td>
<td></td>
</tr>
<tr>
<td>BF stopped because of illness</td>
<td>A time-dependent covariate accounting for whether BF started or stopped or never began for reasons of illness only</td>
<td>To show that illness determined BF behavior</td>
</tr>
<tr>
<td>BF stopped for other reasons</td>
<td>A time-dependent covariate to account for other reasons why BF stopped</td>
<td>Mainly to exclude the illness variable</td>
</tr>
<tr>
<td>Controls</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Age of mother at child’s birth</td>
<td>Child’s birthdate in century months minus mother’s DOB in century months. Categories: &lt;19; 19-29; and &gt;29 years</td>
<td>To monitor selection bias due to age bounds of mother and childcare practices</td>
</tr>
<tr>
<td>Colostrum</td>
<td>Dummy variable indicating if baby was put to breast soon after delivery</td>
<td>Indicates how soon breastfeeding began</td>
</tr>
<tr>
<td>Birth order</td>
<td>Indicated by a set of dummy codes for births prior to the index child, who is breastfed</td>
<td>Omitted category is most prevalent group</td>
</tr>
<tr>
<td>Birth interval</td>
<td>Indicator variables for the intervals &lt;18 months; 18-24 months; &gt;24 months</td>
<td>Monitors the role of gestational age and low birth weight babies</td>
</tr>
<tr>
<td>Background</td>
<td></td>
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<tr>
<td>Education of mother</td>
<td>A set of dummy codes to capture: no schooling; primary; secondary and higher education</td>
<td>A proxy for mother’s knowledge of health and childcare</td>
</tr>
<tr>
<td>Prenatal care</td>
<td></td>
<td></td>
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<tr>
<td>Tetanus injection</td>
<td>Dummy for whether mother was given tetanus injection (=1) at pregnancy</td>
<td>To monitor tetanus infection, a serious infant killer</td>
</tr>
<tr>
<td>Prenatal visits</td>
<td>A dummy variable indicating if mother made a visit during the first trimester</td>
<td>Monitors fetal health until birth</td>
</tr>
<tr>
<td>Place of delivery</td>
<td>A set of dummy codes to indicate where baby was delivered: Hospital/clinic; Home; Other</td>
<td>Home delivery is the category omitted</td>
</tr>
<tr>
<td>Infant details</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Sex of child</td>
<td>Dummy variable indicating sex of the index birth</td>
<td>Can monitor biological differences at birth</td>
</tr>
</tbody>
</table>
time a child breastfeeds is subject to the 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. The statistical procedures adopted address some of the issues raised.

**RANDOM EFFECTS LOGIT MODELS**

In studying a problem such as reverse causality, the scientific issue is the distinction between whether the strongest effect of illness causing death or the indirect effect (operating via cessation of breastfeeding) do have differential impact on mortality. Although children who are weaned in the neonatal period (or never breastfed) because of illness or weakness to suckle may experience equally high risks of death, as in the direct case of illness causing death, it is not clear how much the cessation of breastfeeding (due to illness) contributes to death. In this regard the objective must be to determine whether there is significant association with death through the cessation of breastfeeding. The dependence of the marginal expectation of each variable on health status becomes the focal issue in the analysis. To reduce the influence of confounders, regression analysis must adjust for characteristics operating through cessation of breastfeeding and those directly due to illness.

For a binary data, most people would start with a simple standard logit function for the outcome variable y (death in our case), we guess, but we want to think in terms of conditional models in which causal ordering is paramount. The preferred approach here is to work with conditional models containing random effects. In these models, information from other causes of death \( y_{ik}, k \neq j, \text{say} \) is passed quite naturally to the regression for \( y_{ij} \) via the random effect \( e_{ij} \), without forcing any particular closed form on the dependence of \( y_{ij} \) on \( \{y_{ik}; k \neq j\} \).

In effect, the causal ambiguity is tested first on the data using a random effects logit technique with several different models. The random effects model approach is also useful in accounting for correlation, overdispersion, and subject-specific inference (Kuk, 1995). 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 (Trussell and Hammerslough, 1983). It is therefore appropriate to study breastfeeding behavior in the first 12 months of life. Following the example of Montgomery et al. (1986), 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. 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 birth weight babies is much worse than that for normal birth weight babies (Bern, 1971). Birth weight
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 (Singh et al., 1985). Shared family background may, to a large extent, be due to common uterine environment and to common practices of the mother or family during pregnancy, in which nutrition is of key importance (Leichting et al., 1975).

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 (Palloni and Tienda, 1985). First births are treated separately from other births because they may be subjected to somewhat higher-than-average mortality risks (Hobcraft, McDonald, and Rutstein, 1983). 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 outcome 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 cluster \( i \). A random effects logit model can thus be written as:

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

where \( S \) is the vector of variables representing selection (or breastfeeding) factors, \( M \) the vector of maternal factors (e.g., age at birth), \( G \) the vector of pregnancy factors (like prenatal care), \( D \) the vector of variables representing infant details (e.g., birth order), and \( P \) the vector of background factors (e.g., maternal education). The unobservable random effect \( \epsilon_{ij} \) is assumed to be common to the \( j \)th household in the \( i \)th cluster. The correlation between \( \epsilon_{ij} \) and \( \epsilon_{ik} \) (\( k \neq j \)) and the variance of \( \epsilon_{ij} \) determine the design effects for each sample.

**Survival Curves**

We plotted the survival curves for seven groups of breastfeeding infants, using the Stata procedure "Kapmeier" (StataCorp, 1995): (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:

\[
S_j + \Pi_{j<k} (n_{jk} - d_{jk}) / n_{jk},
\]

where \( d_{kj} \) is the number of deaths at time \( j \) among breastfeeding children and \( n_{jk} \) is the number of observations at risk of death. To obtain \( d_{jk} \) and \( n_{jk} \), we let \( k = 1, \ldots, N \) 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 \( d_{jk} = \sum_k d_{kj} \) and \( n_j = \sum_k n_{kj} \). The procedure merely counts the alive observations and the number dying at each
specified time. Thus for example, if a child died at time $j$, he/she is presumed alive just before time $j$ and is assumed dead just after $j$. This distinction is crucial here because the curves presented below are defined at time $j$ and so a child who dies at time $j$ and was breastfed until death will be categorized as still breastfeeding before $j$ but not after. The problem here is one of status (which is the same for a period of time) versus events (which happen at a point in time).

RESULTS

Figure 2 shows the Kaplan-Meier survival curves for the seven breastfeeding groups defined above. Note, for reasons given above, that the number of women who wean in month 0 or 1 is likely to be small, and of these, none may describe the reason for weaning as, say, "mother ill" (Group 2, Figure 2) or "mother working" (Group 6, Figure 2) so that in the end, there may be no survival curves for some groups (as can be seen from Figure 2). This product-limit estimate of the survival curve produces a descending step 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 is an indication of the fact that should breastfeeding be hampered in this group of children, they are the ones who will survive the least. The graph shows that most of the children in Group (5) died before their first birthday, and all of those who survived the first birthday did not reach their second birthday.

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 child survival is least hampered in these categories. The graph shows on the whole that not only do the influences of the grouping criteria on infant and child survival 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 log-rank statistics under the assumption that the underlying survival curves are the same, using the $\chi^2$ statistic (Peto and Pike, 1973). The results indicate that the groups are indeed different in both their reason-specific and age-specific breastfeeding behavior ($p < 0.01$ in each case). For the control group (still breastfeeding), there were 16 observed deaths, but only 3.68 would be expected if these children had the same survival rate as all other categories of children. This indicates that breastfeeding for a longer period (here, 6 or more months) has a beneficial effect on infant survival.

We now examine the estimated logit models in Table 2 for Nigeria as a whole and Table 3 for Enugu, a subregion of Nigeria, 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 survived the first month of life were less likely to experience conditions that would simultaneously affect their health and their mothers’ breastfeeding behavior. 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 hindsight, one expects effects estimated controlling for health conditions at birth to change estimates considerably in comparison to 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.

**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,
TABLE 2
RANDOM EFFECT LOGIT MODELS OF BREASTFEEDING AND RELATED FACTORS ON MORTALITY OF CHILDREN AGED 0–12 MONTHS BEFORE THE SURVEY: NIGERIA (DHS)

<table>
<thead>
<tr>
<th>Covariates</th>
<th>Model 1</th>
<th></th>
<th>Model 2</th>
<th></th>
<th>Model 3</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>&lt;1 month</td>
<td>0-12 months</td>
<td>1-12 months</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Beta</td>
<td>SE</td>
<td>Beta</td>
<td>SE</td>
<td>Beta</td>
<td>SE</td>
<td>Beta</td>
</tr>
<tr>
<td>Breastfeeding stopped (still=0)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Mother ill</td>
<td>0.273*</td>
<td>0.266</td>
<td>0.232</td>
<td>0.259</td>
<td>0.231</td>
<td></td>
</tr>
<tr>
<td>Insufficient milk</td>
<td>0.437*</td>
<td>0.422</td>
<td>0.388</td>
<td>0.420</td>
<td>0.385</td>
<td></td>
</tr>
<tr>
<td>Child ill</td>
<td>0.719***</td>
<td>0.658***</td>
<td>0.083</td>
<td>0.336**</td>
<td>0.091</td>
<td></td>
</tr>
<tr>
<td>Other reasons</td>
<td>0.510</td>
<td>0.376</td>
<td>0.365</td>
<td>0.298</td>
<td>0.237</td>
<td></td>
</tr>
<tr>
<td>Fed colostrum (no=0)</td>
<td>-0.383</td>
<td>-0.372</td>
<td>0.293</td>
<td>-0.259</td>
<td>0.276</td>
<td></td>
</tr>
<tr>
<td>Birth weight (&lt;2,500g)</td>
<td>-0.344</td>
<td>-0.320</td>
<td>0.397</td>
<td>-0.318</td>
<td>0.392</td>
<td></td>
</tr>
<tr>
<td>Age at birth (18–29=0)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>&lt;18 years</td>
<td>0.237</td>
<td>0.229</td>
<td>0.312</td>
<td>0.220</td>
<td>0.313</td>
<td></td>
</tr>
<tr>
<td>30+ years</td>
<td>-0.138*</td>
<td>-0.135*</td>
<td>0.063</td>
<td>-0.131*</td>
<td>0.060</td>
<td></td>
</tr>
<tr>
<td>Birth order (1st birth=0)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2–3</td>
<td>-0.148**</td>
<td>-0.143**</td>
<td>0.090</td>
<td>-0.142**</td>
<td>0.050</td>
<td></td>
</tr>
<tr>
<td>4+</td>
<td>-0.277</td>
<td>-0.273</td>
<td>0.188</td>
<td>-0.272</td>
<td>0.189</td>
<td></td>
</tr>
<tr>
<td>Previous child died (yes=0)</td>
<td>-0.253**</td>
<td>-0.249**</td>
<td>0.097</td>
<td>-0.232</td>
<td>0.107</td>
<td></td>
</tr>
<tr>
<td>Prenatal care (yes=0)</td>
<td>0.599***</td>
<td>0.563***</td>
<td>0.189</td>
<td>0.524**</td>
<td>0.235</td>
<td></td>
</tr>
<tr>
<td>Tetanus vaccination (no=0)</td>
<td>-0.320*</td>
<td>-0.329*</td>
<td>0.160</td>
<td>-0.332*</td>
<td>0.157</td>
<td></td>
</tr>
<tr>
<td>Education (no=0)</td>
<td>0.129</td>
<td>0.127</td>
<td>0.122</td>
<td>0.123</td>
<td>0.120</td>
<td></td>
</tr>
<tr>
<td>Primary</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Higher</td>
<td>-0.211</td>
<td>-0.213</td>
<td>0.209</td>
<td>-0.217</td>
<td>0.209</td>
<td></td>
</tr>
<tr>
<td>Intercept</td>
<td>0.587</td>
<td>0.419</td>
<td>0.417</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No. of observations</td>
<td>610</td>
<td>3276</td>
<td>3235</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Log likelihood</td>
<td>-236.0</td>
<td>-1020.6</td>
<td>-1008.8</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

*= p < 0.10; **= p < 0.05; ***= p < 0.01
the odds of death increased by a factor of 2.1 (=exp(0.719)) from the baseline value when a breastfeeding child aged <1 month is weaned because he becomes 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 range 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 weaned children who are too ill to breastfeed. Table 3 further shows for Enugu that the odds of death to weaned breastfeeding children who become ill increase by a

### Table 3

<table>
<thead>
<tr>
<th>VARIABLE</th>
<th>0–12 months</th>
<th>1–12 months</th>
</tr>
</thead>
<tbody>
<tr>
<td>Why BF stopped (still BF=0)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Insufficient milk</td>
<td>0.833</td>
<td>0.706</td>
</tr>
<tr>
<td>Mother too ill</td>
<td>0.390**</td>
<td>0.189</td>
</tr>
<tr>
<td>Child too ill</td>
<td>3.283***</td>
<td>0.517</td>
</tr>
<tr>
<td>Other reasons</td>
<td>−0.237**</td>
<td>0.081</td>
</tr>
<tr>
<td>Birth order (1st birth=0)</td>
<td>−0.530**</td>
<td>0.155</td>
</tr>
<tr>
<td>2–3</td>
<td>−0.244</td>
<td>0.193</td>
</tr>
<tr>
<td>Age at birth (18–29=0)</td>
<td>0.370**</td>
<td>0.133</td>
</tr>
<tr>
<td>&lt;18 years</td>
<td>−0.217</td>
<td>0.201</td>
</tr>
<tr>
<td>30+ years</td>
<td>0.473</td>
<td>0.221</td>
</tr>
<tr>
<td>Previous child died (yes=0)</td>
<td>−0.397***</td>
<td>0.090</td>
</tr>
<tr>
<td>No death</td>
<td>−0.519**</td>
<td>0.150</td>
</tr>
<tr>
<td>Prenatal care (yes=0)</td>
<td>0.378**</td>
<td>0.103</td>
</tr>
<tr>
<td>No care</td>
<td>0.359*</td>
<td>0.189</td>
</tr>
<tr>
<td>Tetanus vaccination (yes=0)</td>
<td>0.572***</td>
<td>0.132</td>
</tr>
<tr>
<td>No vaccination</td>
<td>0.553**</td>
<td>0.187</td>
</tr>
<tr>
<td>Education (no=0)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Primary</td>
<td>0.125</td>
<td>0.122</td>
</tr>
<tr>
<td>Higher</td>
<td>−0.233*</td>
<td>0.109</td>
</tr>
<tr>
<td>Intercept</td>
<td>−4.339</td>
<td>−4.294</td>
</tr>
<tr>
<td>Number of cases</td>
<td>1933</td>
<td>1898</td>
</tr>
<tr>
<td>Log likelihood</td>
<td>−202.2</td>
<td>−197.3</td>
</tr>
</tbody>
</table>

**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

<table>
<thead>
<tr>
<th>COVARIATE</th>
<th>Model 1</th>
<th>Hazard ratio</th>
<th>Model 2</th>
<th>Hazard ratio</th>
</tr>
</thead>
<tbody>
<tr>
<td>Still breastfeeding</td>
<td>1.0</td>
<td></td>
<td>1.0</td>
<td></td>
</tr>
<tr>
<td>No illness*</td>
<td>2.3</td>
<td></td>
<td>1.9</td>
<td></td>
</tr>
<tr>
<td>Child ill*</td>
<td>3.6***</td>
<td></td>
<td>0.7***</td>
<td></td>
</tr>
<tr>
<td>Other reasons*</td>
<td>0.5***</td>
<td></td>
<td>0.7***</td>
<td></td>
</tr>
<tr>
<td>Age at birth</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>26 or more years</td>
<td>2.6**</td>
<td></td>
<td>2.5**</td>
<td></td>
</tr>
<tr>
<td>&lt;19 years</td>
<td>0.8**</td>
<td></td>
<td>0.6***</td>
<td></td>
</tr>
<tr>
<td>Birth order</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1st order</td>
<td>1.0</td>
<td></td>
<td>1.0</td>
<td></td>
</tr>
<tr>
<td>2–3 order</td>
<td>1.7*</td>
<td></td>
<td>1.5*</td>
<td></td>
</tr>
<tr>
<td>4+ order</td>
<td>1.6</td>
<td></td>
<td>1.3</td>
<td></td>
</tr>
<tr>
<td>Education</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>No education</td>
<td>1.0</td>
<td></td>
<td>1.0</td>
<td></td>
</tr>
<tr>
<td>Primary</td>
<td>0.7</td>
<td></td>
<td>0.7</td>
<td></td>
</tr>
<tr>
<td>Higher</td>
<td>0.6**</td>
<td></td>
<td>0.6**</td>
<td></td>
</tr>
<tr>
<td>df</td>
<td>9</td>
<td></td>
<td>15</td>
<td></td>
</tr>
<tr>
<td>ΔG^2</td>
<td></td>
<td></td>
<td>22.3</td>
<td></td>
</tr>
<tr>
<td>BIC</td>
<td>3304.8</td>
<td></td>
<td>3310.7</td>
<td></td>
</tr>
<tr>
<td>Number of cases</td>
<td>2082</td>
<td></td>
<td>2111</td>
<td></td>
</tr>
</tbody>
</table>

NOTES: *This model excludes 29 observations for which children were too ill to suckle, but were finally included in Model 2

*Time-varying covariate (excluding children weaned as a result of illness)

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

...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 a child's susceptibility to death more in the younger age segment. 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 further confirm our findings on the question of the magnitude of this influence, an event history approach was similarly applied to the Nigerian and Enugu data sets. The approach was to transform the data with births as observations because at the time of the survey, the duration of breastfeeding had not been completed for some births. For other births, breastfeeding was terminated prematurely by the death of the infant. The event history model results (not presented here but available from the authors) show findings similar to the random effect models.
### TABLE 5
ESTIMATED HAZARD MODELS FOR THE COVARIATES OF CHILD MORTALITY FOR SAMPLE OF MOST RECENT BIRTHS: NIGERIA DHS 1991

<table>
<thead>
<tr>
<th>VARIABLE</th>
<th>0-12 months</th>
<th>1-12 months</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Model 1</td>
<td>Hazard ratio</td>
</tr>
<tr>
<td>Why breastfeeding stopped</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Still breastfeeding</td>
<td>1.00</td>
<td>1.00</td>
</tr>
<tr>
<td>Child ill</td>
<td>1.53***</td>
<td>1.22**</td>
</tr>
<tr>
<td>Other reasons</td>
<td>1.28</td>
<td>1.05</td>
</tr>
<tr>
<td>Fed colostrum</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Yes, colostrum</td>
<td>1.00</td>
<td>1.00</td>
</tr>
<tr>
<td>Waited</td>
<td>1.37</td>
<td>1.15</td>
</tr>
<tr>
<td>Birth order</td>
<td></td>
<td></td>
</tr>
<tr>
<td>1st birth</td>
<td>1.00</td>
<td>1.00</td>
</tr>
<tr>
<td>2-3 births</td>
<td>0.61**</td>
<td>0.60**</td>
</tr>
<tr>
<td>4+ births</td>
<td>1.13</td>
<td>1.13</td>
</tr>
<tr>
<td>Birth interval</td>
<td></td>
<td></td>
</tr>
<tr>
<td>25 months or more</td>
<td>1.00</td>
<td>1.00</td>
</tr>
<tr>
<td>&lt;18 months</td>
<td>1.17**</td>
<td>1.15*</td>
</tr>
<tr>
<td>18-24 months</td>
<td>0.57</td>
<td>0.63</td>
</tr>
<tr>
<td>Birth weight</td>
<td></td>
<td></td>
</tr>
<tr>
<td>&gt;2,500 grams</td>
<td>1.00</td>
<td>1.00</td>
</tr>
<tr>
<td>Less than 2,500g</td>
<td>1.32**</td>
<td>1.09*</td>
</tr>
<tr>
<td>Previous child died</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Yes</td>
<td>1.00</td>
<td>1.00</td>
</tr>
<tr>
<td>No</td>
<td>0.73*</td>
<td>0.69*</td>
</tr>
<tr>
<td>Age at birth</td>
<td></td>
<td></td>
</tr>
<tr>
<td>18-29 years</td>
<td>1.00</td>
<td>1.00</td>
</tr>
<tr>
<td>&lt;18 years</td>
<td>1.21*</td>
<td>1.02</td>
</tr>
<tr>
<td>30+ years</td>
<td>0.55</td>
<td>0.33</td>
</tr>
<tr>
<td>Tetanus vaccination</td>
<td></td>
<td></td>
</tr>
<tr>
<td>No vaccination</td>
<td>1.00</td>
<td>1.00</td>
</tr>
<tr>
<td>Yes vaccination</td>
<td>0.69*</td>
<td>0.54**</td>
</tr>
<tr>
<td>Number of cases</td>
<td>8041</td>
<td>8033</td>
</tr>
<tr>
<td>Log likelihood</td>
<td>-1262.3</td>
<td>-1206.6</td>
</tr>
</tbody>
</table>

**Note:** 'Time-dependent covariate
```
* = p < 0.10; ** = p < 0.05; *** = p < 0.001
```

### SUMMARY AND CONCLUSIONS

It is often concluded that the lack of breastfeeding could be the cause of the elevated mortality among infants. 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 child. Some of the information necessary for analyzing this problem was available for both the Nigerian DHS and the Enugu...
survey. The results presented above show that the risk of mortality is much higher for breastfeeding children who were ill during the neonatal segment than it is for normal breastfeeding children of the same age. 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 any biases imparted by an initial selection mechanism are to the survival of a breastfeeding child would more likely have negative implications for low birth weight and sick babies than for normal birth weight babies. This observation also supports the immunological and biochemical evidence that the benefits of breastfeeding are likely to be greatest in the early part of the first year of life, when the infant's enzymatic systems are not fully developed and its defenses against disease are weak.

In tradition African societies such as those in Nigeria, there are few studies that document whether or not a mother would withhold breast milk from the child in the event that the child is ill. We would imagine that mothers who do not breastfeed their children in the event that the children are ill have to do so as the last resort. The group of children whose breastfeeding had to be curtailed in our sample was selectively small and perhaps biased. They may largely be children whose mothers were not healthy enough to breastfeed. They may also be children who, because of some special disability, could not suckle. The risk associated with such conditions probably exaggerated the survival.

It is, however, important to stress that as useful as the approach to the study may be, it does not completely eliminate the possibility that the reported effects of breastfeeding on survival are confounded by weaning due to illness. The bias may, in fact, be shifted in the other direction. Perhaps a carefully designed panel of data could resolve the issue once and for all.

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