Author's response to reviews


Authors:

Diddy Antai (theangelstrust.nigeria@gmail.com)

Version: 3 Date: 29 September 2010

Author's response to reviews: see over
Response to Reviewers

Reviewer 1 report
Version: 2 Date: 13 June 2010
Reviewer: Amare Deribew

Reviewer's report:
Comments: The paper has improved. Some comments are as follows:

Minor comments:
Abstract:
# Please include some Measure of association in the abstract. Hence modify the sentence which says ‘individual and community level factors.

Author response:
Measures of association have been included in the abstract. The sentence “individual and community level factors” has been deleted as the reviewer requested.

Introduction:
# Please modify the sentence: ‘however these were survey reports rather than empirical studies, neither were explanatory factors identified’. The last phrase needs grammatical revision

Author response:
The last phrase of the above-mentioned sentence has been modified as the reviewer requested.

Methods:
# Please avoid the term reference category. It is well noted in the tables.

Author response:
The term “reference category” has been deleted in the methods section as the reviewer requested.

# It is not clear why the author prefer the middle category as reference in some of the variables.

Author response:
The “middle” category is the category containing the average the percentage of mothers with the respective community-level characteristics within the primary sampling unit. It acts as the mean population in this case.

# The author described that he uses Cox regression? But the result didn’t show any hazard rate (HR). It is the OR which is reported.

**Author response:**

An oversight! Odd ratios (OR) have been replaced with hazard ratios (HR) throughout the manuscript (body of the text and the table 5) as the reviewer requested.

**Results:**

# Please cite tables at the end of the paragraph. Avoid the term ‘Table X’ show at the beginning of a paragraph. Such wording is repeated several times.

**Author response:**

Reference to the tables are cited at the end of the respective paragraphs as the reviewer requested.

# Please include some of the table as annex. You shouldn’t put all the tables under additional file. Additional file is applied for other extra files which may be bulky to include them in the text. Your tables are part of the main results.

**Author response:**

Will be done during submission.

**Level of interest:** An article of importance in its field

**Quality of written English:** Needs some language corrections before being published

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

I declare that I have no competing interest
Reviewer 3 report


Version: 2 Date: 16 August 2010
Reviewer: Douglas Ewbank

Reviewer's report:
This paper presents an interesting analysis of spatial differences in infant and child mortality in Nigeria based on sophisticated multi-level statistical techniques. I have a few questions about the methods that need to be clarified. However, most of my comments involve the specification of the model and the interpretation of the results.

Major Revisions:

# It is not clear to me what the basic method is. The Abstract states that the analysis was based on logistic regressions which suggest a simple analysis of which children were still alive. However, the Methods section states that they used Cox proportional hazards models which take into account person-years of risk and censor children at their current age or at 5 years of age. I assume that he has used a proportional hazards model, but not a Cox model which doesn’t fit the underlying hazard function. These ambiguities need to be cleaned up.

Author response:
This study was based on multilevel Cox proportional hazards analysis, for the exact reasons stated in the manuscript (because it models censored time-until-event data as a dependent variable where one can assume that the covariates have a multiplying effect on hazard rates and warrants recoding characteristics in dummy variables).

# It is also important to describe the age categories used – years, months. It is not clear that a simple proportional hazards model is appropriate. The determinants of neonatal, post-neonatal and child mortality may be very different.

Author response:
The age category has been described as being “0 - 59 months” under “Outcome variable” in the measurements section.
# For example, the effects of type of delivery may not extend beyond the first few months of life. What is the difference between Table 3 and Model 3 in Table 5? Both adjust for region and individual level variables. Are both needed? Also, is it really instructive to include Table 4?

**Author response:**

A, Table 3 controlled for the effect of individual-level characteristics (sex of child, birth order, mother’s age, mother’s age at birth of first child, marital status, mother’s education, and wealth index) on the risks of under-five mortality by region of residence. Model 3 in Table 5 on the other hand was controlled for the effect of individual-level characteristics (sex of child, birth order, mother’s age, mother’s age at birth of first child, marital status, mother’s education, and wealth index) and cross-level interactions (mother’s age at birth of first child & community prenatal care by doctor, as well as mother’s education & community prenatal care by doctor) on the risks of under-five mortality by region of residence.

Cross-level interactions were suggested in the first review process by one of the reviewers.

# The comment on page 3 that there are no “empirical studies” of regional differences in under-five mortality in Nigeria is simply not true. The author’s own previous work examined these differences. DHS reports show these differences. This is not simply a failure to reference other studies; it is a failure to situate the present study in the previous literature. What is it that this study adds to our understanding? How is this study informed by previous research?

**Author response:**

The difference is this! Previous studies, including that of the author, may have “controlled” for region of residence while controlling for other possible confounders. However, these studies were not investigating under-five/childhood risks mortality in the different regions in Nigeria by using region of residence as the main exposure variable. The latter is an intentional assessment of the individual- and community-level factors across regions in Nigeria.

# The author has written about the important role of religious differences in explaining under-five mortality in Nigeria using the same data used here. However, it is not clear why religion is not included here. In fact there is only a vague reference to “ethno-religious situations”. Differences in religion are one of the most glaring differences between Northern and Southern Nigeria. It is likely that religious differences may
underlie many of the behavioral differences such as immunization levels and huge differences in the level of female education.

Author response:
Religion was not included in this present analysis due to problems of co linearity (with ethnicity), given that ethnic distribution is closely associated with religious variation in Nigeria.

# The conclusions seem to suggest that the disadvantages in northern areas disappear (i.e., become insignificant) after adding in the controls. However, in the final model (Model 4, Table 5), the estimates for the three northern regions are virtually identical: 1.25, 1.37 and 1.28. It is likely that replacing these three regions with a simple Northern region would lead to statistical significance. Doing so would significantly alter the conclusions which only emphasize the higher level of mortality in the South South region. The conclusions should not be so closely tied to a fixed regionalization scheme.

Author response:
No empirical public health study in Nigeria has ever reported results grouping the three distinct Northern geopolitical regions into a single category “Northern region”. This would definitely counter all the possible policy implications of epidemiological studies in a regionally, ethnically, and culturally diverse nation like Nigeria (and all the other multicultural societies in the world today). Studies like this help policy makers in both the central Federal government of Nigeria the policy makers in the individual states that constitute these distinct regions. It is for these reasons that any nationally representative empirical studies on Nigeria including region of residence as a variable need to use the 6 geopolitical zones/regions.

# It is easy to see how age at first birth might affect the mortality risk of first births, but how is it relevant to later births? One of the most common control variables in this type of study is urban/rural. Without including this variable, it is difficult to interpret the means of some variables that are probably very different between urban and rural areas, e.g. delivery in hospital.

Author response:
Studies have shown that: i) there is a negative effect of age at first birth on the rates of closely spaced second births especially among women who gave birth at a very young age; ii) teenage
mothers are more likely to delay seeking prenatal care for the second pregnancy compared to teenage mothers who delay their second pregnancy until adulthood; and iii) teenage mothers who have a rapid second birth tend to have substantially poorer socioeconomic and familial outcomes than do those who delay subsequent childbearing.

**Minor Essential Revisions:**

#a The conclusions include the need for changing birth spacing. That may be true, but there is no evidence presented about the effects of birth spacing.

#b The author’s earlier paper (J. Relig. Health, 2009) gives the sample size of the 2003 DHS as 7,620 women. The current paper gives 3,725, but number of live births is the same in the two papers.

**Author response:**

#a This has been deleted!

#b The author’s earlier paper titled “Inequities in Under-Five Mortality in Nigeria: Differentials by Religious Affiliation of the Mother” stated the following in the methodology section:

“Data on all women aged 15–49 (n = 7,620), and all men aged 15–59 in a sub-sample of one-third (n = 2,346) of the households were collected in face-to-face interviews. The data included 3,725 women and 6,029 live born children born within 5 years prior to the survey”.

The current paper on the other hand stated: “This is a nationally representative sample collected by face-to-face interviews from 3,725 women aged 15 to 49 years”.

Out of 7,620 women, data was collected from 3,725 women. This is the same in both papers. The author fails to see the point of the reviewer!

# You need to explain the wealth variable. The conclusions note that it is based on a list of assets, but this needs to be explained earlier.

**Author response:**

This has been clearly explained; see under the “Individual-level explanatory factors” section:

# It might also be used to list which states are included in each region.

**Author response:**
This has been included as the reviewer requested (see under “Exposures” and “Region of residence” sections).

Pg. 10: “Region of residence was included as the only explanatory variable in Model 1 to assess the independent influence of region…..” The word “independent” isn’t what you want to say. Model 1 merely shows the gross effects of region before netting out the effects of other variables. There is only a brief mention of the fact that the intraclass correlation across communities remains significant. The implications of this should be discussed, i.e., many differences among communities still remain unexplained. Note: the “Supplementary” material is really the tables that should be in the paper.

Author response:
This has been changed to read “Community-level variation remained significant after controlling for individual- and community-level variables, indicating that differences among communities still remain unexplained and a need for further exploration of community-levels determinants of under-five mortality”. (see discussion section)

Level of interest: An article of limited interest
Quality of written English: Needs some language corrections before being published
Statistical review: Yes, and I have assessed the statistics in my report.
Declaration of competing interests:
I declare that I have no conflicts of interests.