Reviewer's report


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Reviewer: Douglas Ewbank

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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 suggests 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. 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. 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?

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?

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.
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.

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.

Minor Essential Revisions:

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.

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.

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

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.

**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.