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LETTER

Undernutrition in ART-naïve Nigerian children

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Dear editor

This letter is a response to the recent paper by Anigilaje and Olutola entitled "Prevalence and risk factors of undernutrition among antiretroviral-therapy-naïve subjects aged under 5 years old in Makurdi, Nigeria: a retrospective study", published in the *International Journal of General Medicine*.[1] The relevance and importance of any sound research on undernutrition in HIV-infected children is high, and we strongly support publication of papers on this topic. However, while we appreciate the health awareness that could be generated by this paper, we have some concerns regarding this study.

The title of the article and the labeling of its tables indicate that the study population included children aged younger than 5 years, but it is also stated that the selection criteria included HIV-infected children ≤15 years. Further, it mentioned in Table 1 that the total sample was 182, and only children with undernutrition were included.

In the methods section, it is stated that upon enrollment into care, all parents or caregivers of HIV-infected children were required to provide written informed consent for use of their data for research purposes. Informed consent is voluntary for any research, and should not affect the care provided. It is not clear in the present study if the informed consent was voluntary.

The results report the prevalence of undernutrition to be 12.1% (22/182), 33.5% (61/182), and 54.4% (99/182) for underweight, wasting, and stunting, respectively. What is actually being reported here is the proportions of underweight, wasting, and stunting among undernourished children, but to estimate the prevalence of undernutrition, the study sample should have included all HIV-infected children younger than 5 years irrespective of their nutritional status. In order to identify risk factors for undernutrition, comparisons should have been made between each of the three undernourished groups and children without undernutrition, and not between the undernourished groups themselves.

The table captions appear to be mislabeled. Table 2 denotes the prevalence and risk factors for wasting and not for underweight (sample size is 61, not 22) and Table 3 shows the prevalence and risk factors for underweight and not for wasting (sample size is 22, not 61). This mislabeling has resulted in erroneous interpretation of the results and hence the discussion of the findings.

In the tables, the crude odds ratios for some variables show a significant *P*-value, but the adjusted odds ratio and confidence interval for two variables are not included (see mode of HIV transmission and caregivers' HIV status in Table 2 and caregivers' HIV

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status in Tables 3 and 4). It is also observed that the odds ratio and confidence interval for some variables are missing, but the crude odds ratios have been mentioned with a *P*-value as 0.000 (modes of infant feeding in Tables 2–4). Some variables are significant risk factors based on the crude odds ratio, but are significant protective factors after multivariate analysis (caregiver on highly active antiretroviral therapy, marital status of caregiver, tuberculosis, and esophageal candidiasis in Table 2, and age and CD4 count in Table 3). The justification for this major difference is not stated in the article. The authors have included all variables achieving a significance of 0.1 in the multivariate analysis. It is advisable to include only those significant variables that could be confounding to the variable of interest. When interpreting the results, the authors state that "the trend was such that female subjects were 0.292 less likely to be underweight". This result could be better reported as females being 71% less likely to be underweight than males.

The authors attribute the lack of a significant association in multivariate analysis between "wasting" (corrected here to "underweight") and the tested variables to the small sample size. It would be appropriate to mention if the sample size was estimated before the start of the study and whether the available size was adequate.

The authors state that the lack of data on HIV-negative children in a similar setting limited their interpretation of the results. This is not a study limitation, because the authors did not intend to include HIV-negative children; however, this could be a recommendation for future research. Considering the public health importance of this issue, studies with more methodological rigor are recommended in order to draw concrete conclusions for clinical practice.

Disclosure

The authors report no conflicts of interest in this communication.

Reference

1. Anigilaje EA, Olutola A. Prevalence and risk factors of undernutrition among antiretroviral-therapy-naïve subjects aged under 5 years old in Makurdi, Nigeria: a retrospective study. *Int J Gen Med*. 2015;8:131–141.

Authors' reply

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Dear editor

We are delighted to see that our paper is having an impact as far afield as the United Arab Emirates, and take the opportunity to respond to the above comments made by Shantakumari et al. First, we apologize for the typographical error stating "age ≤15 years" to be a study inclusion criterion; this should have read "≤5 years", and is to be corrected.

Our total sample size was 182, and did include subjects with and without undernutrition. As can be seen in Table 1, the proportional prevalence of the three types of undernutrition were not mutually exclusive, as the focus of the study did not include a composite index of undernutrition. A close look at Table 1 shows that the proportional prevalence of each of the three types of undernutrition was derived from the total sample size of 182. In other words, some subjects who were underweight may have also been stunted, so the proportions are not mutually exclusive. It is because of this limitation that researchers now talk about a composite index of anthropometric failure of undernutrition, which takes into account the aggregate of all types of undernutrition, including underweight, wasting, and/or stunting. We refer readers to this concept, which is a necessary prerequisite for understanding Table 1.

With regard to the consent issues raised by Shantakumari et al, ie, the seeking of written consent for the use of the children's data for future research purposes being sought from parents/caregivers, we stress that assurances were given to these parents/caregivers that their decision regarding participation in this research would in no way affect the care received by their children.

We agree that Table 1 is mislabeled, and should be corrected by substituting "wasting" for "underweight". However, we do not believe that the labeling error in Table 1 would detract from the validity of our results, discussion, or conclusion. It should be noted that Tables 2 and 3 are not mislabeled. We also acknowledge the absence of some adjusted odds ratios and confidence intervals in Tables 2–4. We are of course correcting this oversight, although the corresponding crude odds ratios with P=0.000 are mentioned (see modes of infant feeding in Tables 2–4).

Shantakumari et al are correct in their observation that some variables in Tables 2 and 3 are significant risk factors based on the crude odds ratio but emerge as significant protective factors after multivariate analysis. The reason a multivariate regression is performed is to determine the impact of a variable on an outcome variable while testing the interaction with other confounding variables that also impact on the outcome of interest. Therefore, it is possible for a variable that is a significant risk factor on bivariate analysis to become a protective factor after adjusting for confounders.

Regarding the observation that we only included variables with P-values of 0.01 in the multivariate analysis, our statisticians used P-values of 0.1 (10%) and 0.2 (20%) to accommodate as many of the confounding variables that may impact on the measured outcome of interest, as possible. All the factors included in our multivariate analysis are confounders for undernutrition. In some cases, these confounders are "a priori" factors that have been documented to impact on outcome variables in studies in other settings. When this is done, especially for a study with a small sample size, it gives a strong interpretation of the level of statistical significance of the confounding variable in question, especially when found to be significant in multivariate analysis. It reduces the residual variation and decreases the standard error of the regression coefficients for many confounders, as far as possible.

We concede that "the trend was such that female subjects were 0.292 times less likely to be underweight" could have been expressed as "females are 71% less likely to be underweight than males", but consider this to be merely a matter of semantics.

To address the comments by Shantakumari et al regarding our failure to include information on estimation of sample size and whether our sample size was adequate, we point out that this was a retrospective study of patients attending one clinic over a specified period of time. Calculation of sample size is generally not required for retrospective studies, but we would certainly agree that this is one of the limitations of this type of research.

We are in agreement with Shantakumari et al that non-inclusion of data on HIV-negative children is a potential limitation of our present research, in that our results do not accurately represent the burden of undernutrition

in HIV-infected children under 5 years in our setting. However, investigation of HIV-negative children was not part of the aim of our study. We also agree with Shantakumari et al that inclusion of an HIV-negative group could be a recommendation for future research. Our specific recommendation in this regard would be for a longitudinal study that takes into account the burden of undernutrition in children not infected with HIV in our setting.

Disclosure

The authors report no conflicts of interest in this communication.

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