Reviewer's report

Title: Survival Status and Predictors of Mortality in Severely Malnourished Children Admitted to Jimma University Specialized Hospital from 2010-2012, Jimma, Ethiopia: A Retrospective Longitudinal Study

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Reviewer: James Bunn

Reviewer's report:

Major compulsory revisions

Methods para 1
I was unclear if a retrospective data analysis (which would explain the missing records) or a prospective study (with data captured each day). Can this be clarified?

If this unit serves a population of 15 million, it would be helpful to know if a tertiary unit taking referrals from other centres or CMAM programmes, or a traditional NRU. I would be surprised if a population of 15 million only has <500 children with SAM a year, so assume other providers are managing SAM in this population. And are children with comorbidity managed on general wards (so not captured) or are the vast majority managed on the NRU?

para 4
There is a need for far more detail on the methods used generally. And specifically on the comorbidities and clinical features. Were this collected from a pro forma data capture sheet used by clinicians, or only where the clinical notes made comment on presence or absence of a sign/symptom. Given the relatively large proportions with dehydration, or shock, criteria for assessing this is important as often difficult clinically to determine. Quality of the clinical data is difficult to assess with the methods as currently given.

Definition of cure is important as there is confusion in terminology later between 'improvement' and (nutritional) 'cure' as outcome.

Data analysis
See stats comments later

Results
There are numerical problems in this section.

para 3
If marasmus found in 83.8% of children 0-59 months, I cannot see how oedematous malnutrition can be present in 72.2%. I think (from the figure) they are referring to the proportion of marasmus and kwashiorkor in each age group,
but this is not how it reads. Crude numbers would help generally with type of issue

The discussion in this section is (as written) confusing as indicates marasmus more frequent in under 5s due to diet and infection factors. In fact kwashiorkor is a problem that classically occurs in under 5s, and not in older children.

I was surprised only 58% of 6-59 m (the standard range, not 60m as stated) had a MUAC<11.5cm, as this is a NRU, and apart from oedematous malnutrition this would be expected in almost all other admissions. Can the authors describe the nutritional status of the children admitted to the unit more clearly.

para 5
This section is vague, and clinical features need more definition. For example deranged respiratory rate was found in 69%. Is this using WHO IMCI ranges? and if this high indicates the unit is predominantly admitting sick children with complicated severe acute malnutrition (SAM). 6.4 % in shock and 12.5% impaired conscious level, and 88.8% with watery diarrhoea similarly indicates a very sick population if correct. This sentences needs clarifying (were those with watery diarrhoea a subset of the 66% with diarrhoea. And was this status on admission or occurrence during admission.

para 7
How was disseminated TB diagnosed. As almost 10% diagnosed with this? Anaemia needs defining - was this pallor or a haematocrit/ Hb? Method for HIV diagnosis (rapid ELISA I assume) should be stated.

para 8
how was comorbidiy after admission handled in analysis. Were the 12% who developed shock in addition to the 6.4% on admission (para 4). In which case either this is being over diagnosed, or is a very sick population of SAM children. And not characteristic of most NRUs. The mortality rates <10% do not suggest the children are this sick

Treatment outcomes
This whole section should be reduced and less discursive. Comparing each parameter with the literature is not necessary, and makes it less easy to see the primary data being presented. Some comments on the positive nature of the programme at JUSH could be moderated, or omitted.

para 1
Given the number of children who absconded 12.9% is greater than the number who died 9.3%, the authors should make estimates of mortality assuming all absconding children died (22.2%) (or explain why they did not - such as follow up data locally). This would give a very different interpretation to the results if (as is commonly found) children abscond as parents find the child is not improving.

para 2 /3
'cure' needs to be defined. There are various definitions for this, but should be stated which was used. I am not clear if cure was the outcome or 'improvement' (para 3 states only 226 [30.6%] met 85% WH).

In para 3 and 4 the potential for discharge to a health centre is comments on for JUSH. Was this an additional outcome not captured in the paper? And if so could it be enumerated? And I assume if discharged prior to 'nutritional cure' there will be further potential mortality after transfer.

The last sentence of para 3 indicates outcomes at JUSH better than in the literature. I am not sure I have interpreted the data presented in the same way, but accept this may be due to these being children with complicated SAM.

Section 5.1 (analysis) I note other sections not numbered
table 1
this indicates that sicker children are more likely to die, as expected
table 2
the mean survival in days column does not make sense, so should be corrected
And without a table of admission characteristics (as numerical data) is is hard to understand the tables, as numbers are hard to access easily from the text.

I am not clear why we have 2 tables with bivariate analyses, and it would make sense (if needed) to present that which is used in the multivariate analysis. The presentation of the statistical table could be clearer, possibly combining crude numbers, univariate analysis and multivariate analysis in one table.

multivariable analysis para 2
the normal pattern is for more deaths in younger children, so comparison with Bachou's study (ref 11) where deaths were similar in >24 months is not a representative paper of compare to.

Again I do not think each variable needs to be compared with the literature, unless the data is different in an important and unexpected (and coherent) way. A comment to say these findings are in a agreement with the literature would be sufficient. And would make easier to read

Conclusion
I would remove the comment on meeting the minimum standards, as I am not sure this was the intention of the study, and could be challenged. This is not indicating other than you had high abscond rates, low nutritional cure rates (against 85% WH) and this may reflect the disease severity in your population.

Graphs/ figures. These would need captions if included, but I do not think are needed and for some the numbers are low. Possibly a survival curve (including absconds)

Minor Essential revisions
Background
para 4
The references provided could be improved, and more recent estimates of the mortality related to malnutrition used (and total child deaths). The Lancet Malnutrition series (Black 2008) would be a better source than that of Collins et al (ref 8). And for total child deaths the UNICEF have the most widely accepted figures. Both of these would give lower values than those in the paper.

para 6
There are references to local protocols for mortality rates (ref 13) and a more authoritative source would be preferred. Reference 11 is not a valid reference to use for increasing admissions in SSA, and I am not clear that the numbers of admissions to NRU type units are increasing as stated. Particularly now CMAM units are available

para 7. Total numbers of deaths are stated as 1.5 million deaths with MAM, and 3.5 million with SAM (not including kwashiorkor). This is far larger than given in more recent publications. (see Lancet malnutrition series e.g. Black 2008)

Para 8
The objectives are not novel and are routinely collected data in most programmes. The authors should be clear what is the intention of the study, as there is a difference between listing characteristics in children who die, and predictors for mortality which may be amenable to intervention. Clarity on the objective, and its use would improve the paper, and take it to a level above a descriptive account. It would help to know what data was 'on admission' so could be used as a means of triaging or prioritising care, and what clinical data was an occurrence any time during admission

Treatment outcome
para1
I would expect the Sphere standards to be cited, rather than ref 8.

Discretionary revisions
I would try to separate out the results from the discussion, and a table of admission characteristics might be helpful prior to the statistical analyses.

paras 1-7 results section
These are rather discursive, and the comparison to other units could be briefer

Level of interest: An article of limited interest
Quality of written English: Needs some language corrections before being published

Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.

Declaration of competing interests:
I declare that I have no competing interests