Author’s response to reviews

Title: Determinants of healthcare seeking for childhood illnesses among caregivers of under-five children in urban slums in Malawi: a population-based cross-sectional study

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Author’s response to reviews:

The Chief Editor
BMC Paediatrics

Dear Sir / Madam

REVISION OF THE MANUSCRIPT BPED-D-19-00174 IN BMC PAEDIATRICS
I write on behalf of my co-authors to thank the Editor and the reviewers of our manuscript titled Determinants of healthcare seeking for childhood illnesses among caregivers of under-five children in urban slums in Malawi: a population-based cross-sectional study, for their insightful comments which have helped to improve our article.

We highlight below how we have addressed each of the comments made by reviewers.
I hope the current version will be deemed suitable for publication

Reviewer reports:

Helena Hildenwall (Reviewer 1): This is a paper aiming to identify potential barriers for accurate care seeking for sick children residing in urban slums. While the topic is of high interest to identify challenges specific to vulnerable children in poor urban areas, I think there are a number of issues in the current manuscript that needs to be clarified. Specifically, I am not sure the definitions used are appropriate to get accurate results on issues for adequate care seeking. First of all, it does not make sense to argue that ALL children with whatever symptoms of illness should see a health care provider within 24 hours. It is well valid for suspected malaria and has, as the authors rightly write, been encouraged for any fever due to the potential rapid development of serious malaria. Your results also indicate that the studied communities have absorbed this message. However, what kind of symptoms was otherwise asked for? It would not make sense to expect guardians to hurry for care with children presenting with a runny nose, a single episode of diarrhea or a simple cough and if these children show up to often at health facilities they may even pose a threat to others with actual health care needs if taking to much attention from them. Thus, your manuscript requires more information on the kind of symptoms children presented with and also for how long.
Response: We appreciate this line of argument. Our interest was to ascertain care-seeking for common
childhood symptoms that are commonest causes of child morbidity and mortality. We have made this explicit by clarifying sentences on lines 108 and 109. We have also added a section to describe our symptom definition on the questionnaire as informed by the Integrated Management of Childhood Illness (IMCI) approach (see lines 218-231). Our description of morbidity and subsequent care-seeking only focusses on these symptoms which child health policy in Malawi encourages prompt healthcare seeking given they account for many child morbidity and mortality. For example, diarrhea was only classified to be diarrhea if loose stools exceeded three times within a specified period – considered as a risk for dehydration hence need to visit health facility (with the view that better the assessment of extent of dehydration to be determined by a qualified health worker especially given that even advice for home management can be provided from there if not deemed severe enough). We hope that our section on symptom definition provides some clarity for readers that gravity of each of these symptoms necessitated seeking healthcare.

With regard to the prompt healthcare seeking (within 24 hours) our view is based on the premise that Child health policy advocates for prompt healthcare seeking for these symptoms. We appreciate the complexity of determining what is ‘prompt’ especially in the context of studies which collect information on phenomenon of interest that has already happened. We are also mindful that unless we are directly observing it is difficult to ascertain what is good enough time. However given the known fact of fast progression of disease among young children, it is desirable that healthcare from a biomedical provider is sought promptly. In this regard, it is our view that prompt seeking using 24 hour period is appropriate especially that there is at least a resolution that backs this for ‘fever’ in view of Malaria being endemic in Malawi and that we could use these for other symptoms also responsible for common child morbidity and mortality. This time period is also backed by evidence from other peer reviewed articles that we have cited.

This notwithstanding, we have added some sentences which we believe provide some clarity on these issues

Information on outcome would also assist the understanding of whether care-seeking was needed - was the proportion of poor outcome higher in the group that did not seek care? If not, what does your findings really tell?
Response: This is noted and we appreciate the value of understanding the patient (child) outcomes after seeking care. However, at the level of baseline (cross-sectional in nature) of a longitudinal study we did not plan for a patient outcomes after seeking care as a measure for this study. Obtaining outcomes after care-seeking was beyond the scope of this baseline component of the study albeit it was considered in longitudinal study outcomes (which we are yet to publish)

Another issues is the time definition. How could you get accurate estimates of care-seeking within 24 hours? More info on how you assessed start of symptoms and time of care-seeking would be needed.
Response: We agree on the complexity of this estimation and acknowledge that we may not have provided details on how this was undertaken or indeed to accept the challenges faced. We have included additional information on how this was done (see lines 202-211 under variable definition and 394-399 under discussion sections)

I am further concerned you provide no info on potential barriers related to the actual availability of health care except from distance: perceived quality of care, opening hours, transport availability, etc
Response: Thanks for the comment. The intention of the study was to focus on demand side factors (i.e. those that are related to caregiver and household) and that is why we felt the need to explicitly mention in lines 109-112 citing “Our research contributes to literature in understanding the dynamics of child
healthcare seeking among caregivers of under-five children focusing on demand side factors in the context of an urban slum in a developing country”. We appreciate the comment and agree on the importance of supply side factors thus in our other published studies conducted in the same study sites, among a subset of the same study population and which we have referenced in this paper (see Ref 37 and 51) we outlined the supply side factors deemed relevant in this context (see lines 101-107) which included the following factors: “attitude of health workers, distance to health facility, thoroughness of health examination, availability of medicines and supplies, cost of healthcare have been cited to influence care-seeking decisions for sick children among the urban poor and slum residents in Malawi”

I think the above are main challenges to the interpretation and importance of this paper and question whether it can be published without a thorough clarification and possible (recommendable) reanalysis. We hope our responses to the comments and the modifications made as per reviewers comments are satisfactory to merit publication

Some additional comments:

Overall
It seems the study has been done within the framework of another, longitudinal, study. It would be very useful to know more about this study to understand the context. Pls add reference(s).
Response: The longitudinal study has not yet been published in a peer reviewed journal thus reference could not be provided. Details of the longitudinal study are presently only in a PhD thesis and we felt it would not be an appropriate reference for an article in a peer reviewed journal. However, we have made efforts to provide additional details where possible, such as the sampling framework

Please add information on the study areas - why were they chosen, how do they differ from other settings in Lilongwe.
Response: We have added some information pertaining to rationale and process of choosing the study sites - see lines 122-129 (i.e. they fit the slum description as the primary geographical interest of the study and the process of selection being random selection – more details are also now referenced to our previously published paper).

It seems odd to me that you can provide a distance in kms to health center since I would assume the included areas are big and different parts of them are differently far away from health centers?
Response: Indeed. Our previous estimate of distance was a metric measure from what was perceived to be the central part of each study area to respective health facility in question. Granted that the households are geographically spread we appreciate the comment in that the estimate may not accurately represent the positioning of each household relative to the health facility (unless we had done a GIS). For purposes of clarity, we have deleted sentences that indicated distance estimates in kilometres

It is unclear who you identified as the study subject - was it the guardian or the child or a unit of both of them?
Response: We used the caregiver and child as unit given that questions were asked from the caregiver but about the child. We have added a sentence that clarifies this (see lines 178-179). Additionally, lines 143-144 under eligibility criteria citing under-five children and caregiver; and lines 150-151 and lines 161-162 make an attempt to clarify this point as they make reference to pairs of under-five children and caregivers.
It seems you have not considered that all interviewees will not have a sick child in your samples size calculation?  
Response: We believe that this was considered given that we used a standard method of sample size calculation that included an estimation of \( p \) in the formula

You mention that the tool has been "validated". Please provide an explanation and a reference to how this has been done.  
Response: We have added lines 182-186 which we hope bring clarity as suggested. Our line of thought in this regard is that questions were adopted from Demographic and Health Survey questionnaire and a survey on Community IMCI – these have previously been pretested in Malawi and have been used as standards internationally with confidence on their internal validity

For the tool, please clarify how data was collected - did respondents fill the form or were they asked questions?  
Response: We believe that lines 177-178 provide this information “Face to face interviews in local Chichewa language were conducted by research assistants who had undergone training in the study protocol and interviewing techniques”. We have however added lines 179-180.

How long time did interviews take?  
Response: We have added a sentence to include the time it took to administer the questionnaire (see lines 180-182)

Data collection was only conducted over three months so changes in disease patterns and potential care seeking challenges (floods during rainy seasons etc) have not been considered. Pls add to limitations.  
Response: This is true and reflects the general weakness of cross-sectional studies. Ascertaining seasonal patterns of disease and care-seeking was beyond the scope of this cross-sectional study albeit it was one of the areas of interest for a longitudinal study. However, we have included a sentence indicating this as a limitation (see lines 502-505)

In abstract you say data was analyzed using "descriptive and logistic regression" - please change writing since currently sounds you have done something called "descriptive regression".  
Response: The description has been changed to descriptive statistics and logistic regression analyses

In results you mention on line 274 that in total 85% reported illness, however that does not correspond to the reported proportions in the individual groups that are all less than 85%. How is that possible?  
Response: We have gone through this and confirm that it is possible. We report 85% as reported illness for any of the three symptoms of interest. At 77% fever was the most prevalent symptom. An additional 8% only had either diarrhea or ARI symptoms hence it is possible to have 85% for any of the three symptoms of interest and have individual symptoms to all be less than 85%

Line 347 - reference to personal communication. Pls add with whom.  
Response: We have included an extension to this by indicating ‘Personal Communication with Official from Ministry of Health” (see lines 381-382). For purposes of anonymity we have not included the position of the person (as the person works for a lean department and could easily be identifiable should we even mention the name of the department within Ministry of Health)

Not sure about journal guidelines but it seems you may have too many references?  
Response: Journal Editor addressed this issue
Kimberly Arcoleo (Reviewer 2): Thank you for the opportunity to review this manuscript. This paper examined factors affecting healthcare seeking behaviors among a sample of caregivers with children under 5 years of age living in urban slum neighborhoods of Malawi. The Integrated Management of Childhood Illnesses (IMCI) is the primary main approach to promoting access to healthcare across a continuum of care that is being used to increase access to essential child health services in support of the Sustainable Development Goals. To date, this has been implemented primarily in rural areas with the assumption that urban areas had better facilities and programs. The premise is that in developing countries urban environments have typically been examined as homogenous entities but this ignores the fact that there are urban slums within these neighborhoods which have higher rates of health disparities and poor health outcomes. Thus, these investigators sought to identify factors influencing caregivers' decisions about when to seek healthcare, the timing of the healthcare visits, and under what circumstances they sought care. These analyses used data collected as part of a larger population-based study.

The study design was rigorous and included three urban slum neighborhoods in Lilongwe, Malawi, none of which included a public health facility. An appropriate sample size calculation was done using an acceptable attrition rate. Probability Proportionate to Size sampling was used which is acceptable. A pretest of the questionnaire was conducted prior to the main study. An important independent variable, caregiver autonomy, was measured because in many cultures, health care seeking and treatment may not be independent decisions.

The results of this study highlighted some critical areas that could be become targets for intervention and also demonstrated that the IMCI model should be considered for urban slum areas as a mechanism for improving early detection and treatment of disease in infants and young children. The results of this study also point to the need to disaggregate data into smaller geographical units because one cannot assume that an urban or rural area is homogeneous within the boundary that is defined.

This study was well-done and the manuscript well-written. It provides valuable information to inform health education strategies for caregivers and families and identifies potential areas for programmatic and policy change.
Response: Thanks for the review and the comments

Yours faithfully

Edgar Arnold Lungu, PhD