Author's response to reviews

Title: Evaluation of the Mercy weight estimation method in Ouelessebougou, Mali.

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Version: 3 Date: 9 March 2014

Author's response to reviews: see over
Dear Editors,

We are very grateful to the peer reviewers for their thorough and favorable review of our manuscript entitled “Evaluation of the Mercy weight estimation method in Ouelessebougou, Mali”

Please find enclosed the manuscript amended according to the peer reviewers’ comments together with responses to each of the comments. Revised section in the manuscript and response to reviewers’ comments are highlighted in blue.

We hope that these sufficiently address the reviewers’ queries and comments and look forward to receiving your final decision.

Sincerely yours,

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Responses to reviewers’ comments

Reviewer 1

Minor essential revisions:
1. APLS is evaluated in this study. In 2010 the weight estimation formulae used in teaching material for this was changed; your references are prior to this, where the formula \((\text{age}+4)\times 2\) was solely used. Was the ‘old’ or ‘new’ formula used? If the prior formula was used then this should be clearly stated (ie as pre 2010), or compared against current APLS formulae.

   We have revised the analyses to reflect the APLS equations published in 2011. The APLS reference has also been updated.

2. You state in the results section that the weight distribution for the population is negatively skewed. Fig 1 shows a positive skew for weight distribution.

   The descriptors for the separate panels in Figure 1 had been transposed. We have corrected the statements related to which distribution is positively and negatively skewed in the text.

3. In the results section you state "In comparison with the mercy method, none of the comparator weight estimation methods predicted weight in 100% of the participants enrolled in this study". Why were the comparator weight estimation methods not able to estimate weight in all participants? Please clarify.

   The comparator methods have age or length restrictions that preclude their application to a subset of the pediatric population. We have attempted to clarify this in the Results, paragraph 2.

Reviewer 2

Major Compulsory Revisions
1. The authors stated that “It appears that the MM can be used in West African children without modification extending the utility of this weight estimation strategy”. To which extend do the author think this result (Ouensssetbougou, Mali) can be extrapolated to the entire West Africa region? The issue should be discussed.

   We feel that the excellent performance of the MM in Malian children, comparable to those observed when this method is applied to children in the U.S and India, indicated that the utility of the method goes beyond the west-African children. The discussion has been edited to reflect this.

2. Elaborate more on sample selection (i.e method of selection).

   The “Subjects and Study Design” section has been expanded with more detailed information on the participants selection.
3. Performance measures are of concern in this paper: a) Correlation coefficient: Correlation is not always a good measure of performance in this case; e.g. if Y is twice X, then Pearson’s correlation = 1. b) Linear regression: By choosing to regress the predicted weight on actual weight, one should check that the intercept is statistically null and the slope is not statistically different of 1. For all methods, only the confidence interval (CI) of Nelson contains 0, thus intercept is not statistically different of 0. For slope, none of its CIs contain 1, thus all these slopes are statistically different of 1. c) ‘Mean error (ME) was calculated by taking the difference of the predicted and actual”. It is actually just a difference, not a mean; it may just be called error or residual (as it is commonly called in Statistics). d) “Mean percentage error (MPE)”, not convincing as measure of performance e) Root mean square error (RMSE) : may be acceptable I would suggest that, in addition to Root mean square error (RMSE), the authors choose two others measures of performance: the “limits of disagreement” (see e.g., Indrayan, 2008, Medical Biostatistics, page 564-566) and intraclass correlation (for each method, may be after log-transformation).

*We agree that regression offers the least valuable information so we have removed these analyses from the manuscript. For clarification, mean error (ME) has been changed to residual error (RE) and mean percentage error has been changed to percentage error (PE). We have examined Indrayan’s description of “limits of disagreement” [Medical Biostatistics, 3rd Edition, 2012, pp 653-655] and believe that this statistic is accurately represented by providing the range for residual error and percentage error which we have now included in Table 2. We have also included the intraclass correlation coefficient for each method in Table 2.*

4. Authors should explain why the final sample size depends on the estimation method, e.g. 473 for Mercy; 269 for APLS; 446 for ARC, 365 for Broselow; 369 for NELSON.

*This is now addressed in the Results, paragraph 2.*


*The reference has been incorporated into the manuscript.*

Minor Essential Revisions

1. Page 4, Introduction, 3th paragraph, “In response to the lack of a robust, broadly-applicable weight estimation method for children we recently developed the Mercy Method [21]”. The ‘we’ should be changed since the current authors are not all those who developed the method. That method was developed by Abdel-Rahman and Ridge [21].

*“We” has been changed to “investigators.”*
2. Methods, 1st paragraph, page 5, Write “years” instead of “yr”.

“Yr” has been changed to “year.”

Discretionary Revisions
1. Since “This study was undertaken to examine the performance of the Mercy Method in a West African pediatric population”, it would have been better if this study were multi-center.

We agree with the reviewer that more centers would have been better; however, the resources provided were relatively restricted. The excellent performance of the method in children from very very different population and environment such as US, India and Mali suggest that it could be use for sub-Saharan African children who are more Malian children. The discussion has been edited to reflect this.

2. Is there a specific software for Mercy?

At present, there is no software available for Mercy.