Author's response to reviews

Title: Comparison of air displacement plethysmography to hydrostatic weighing for estimating total body density in children

Authors:

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Dear Mr. Newmark;

My co-authors and I have completed the revisions suggested by the reviewers. Enclosed you will find the updated manuscript, "Comparison of air displacement plethysmography to hydrostatic weighing for estimating total body density in children" for resubmission and consideration for publication to BMC Pediatrics.

We feel with the submitted revisions that you and the reviewers will find that the manuscript is ready for consideration for publication. Below you will find response to both reviewers in point-by-point fashion. If there is any further information needed, please do not hesitate to contact us to expedite the publication process.

Correspondence regarding this manuscript should be directed to:
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Sincerely,

Holly R Hull, MS

We have had an opportunity to review the comments from the two reviewers. We feel that we have adequately addressed both reviewers comments with a point by point response to each issue. We have made substantial changes to the discussion and concluding statements and now feel that our paper is more in line with the actual data. We hope that the reviewers and editor will now find that the paper is acceptable for publication.

Comments to Reviewer: Dr. Randy Clark

MAJOR COMPULSORY REVISIONS
1. We apologize for any "duplicate word-for-word phrases from previous work", consequently we have made every attempt to ensure "wording" and "phrases" used in this manuscript are unique to this paper.

2. We re-wrote the findings and conclusions as it pertains to "provocative" and gender differences. We hope now that the reviewer will find it acceptable.

3. The author wanted justification and clarification for using simultaneous residual lung volume in children for this study. In (1998) "Body composition by air-displacement plethysmography by using predicted and measured thoracic gas volumes" J Appl Physiol. 1998 Apr;84(4):1475-9 McCrory et al., showed that as a group, males and females (adults) a ~200 ml difference existed between using a predicted vs. measured simultaneous residual lung volume, this resulted in a 1.4% fat difference. We would argue that it is always best to measure a physiological variable (in this case residual lung volume) vs. estimating it. To our knowledge the reviewer is correct in saying little if no data exists investigating the difference between a simultaneous residual lung volume vs. using a predicted residual lung volume equation in children, thus necessitating and crucial that we measured the residual lung volume instead of predicting it. Others have used simultaneous residual lung volume in children and have reported generally that it was well tolerated (DeStefano RA et al titled "Changes in body composition after a 12-wk aerobic exercise program in obese boys" Pediatric Diabetes Volume 1 Issue 2 Page 61 - June 2000 doi:10.1034/j.1399-5448.2000.010202.x).

4. The reviewer is correct to say the TGV and RV are not the same --- when we said "analogous" we meant to imply that the respective lung volumes for the TGV and RV must be measured or taken into account, but not "figuratively the same". We apologize for the confusion and have made changes to indicate that the TGV and RV are not the same, i.e. we removed it from the paper.

5. Excellent point, we over-stated our findings since we did not have enough power to detect any differences. Consequently we have changed that and now the manuscript accurately reflects this and the "strong" wording has been changed and taken out (that gender differences exist). See #2, we think the paper now more accurately depicts our findings.

6. Since we re-wrote our findings (see #2, 5) we feel your comment now becomes mute (i.e. we don't say it is valid).

7. We felt the results were provocative because of the gender difference observed, however we took this out (see response to #2,5,6).

8. Good point about R2 being a measure of precision, we have re-worded and said "the amount of shared variance between HW and ADP".

9. We feel that by re-writing the results, discussion, and conclusions our findings are in-line (i.e. by backing off of the strong comments) with our data. As a result, I think your concern is "mitigated"

10. We appreciate the reviewers interest in the subjects body temperature and BSA in the "potential error" on the BOD POD and we agree that body temperature does play a role (~1%), however we never measured it, thus we can not comment on it. However, if the reviewer would like, we can add a portion attesting to its role in the validity of the test.

11. No, absolutely not --- these children are entirely different than the ones used by Fields and Goran in 2000 (those kids were in Birmingham) these kids are from Oklahoma tested in 2004 (November and December) / 2005 (January).

12. Good point again, we have backed off our comments about HW and we feel the paper sheds a more "positive" light on HW given our data and study results.

Comments to Reviewer: Dr. Jonathan C. Wells

MAJOR COMPULSORY REVISIONS

1. Admittedly, direct carcass analysis is the "Gold Standard". We have removed the statement that "HW" is the Gold Standard.

2. We deleted reference to Dewit.

3. Thank you for this oversight, we have corrected this omission.

4. We have removed this statement concerning the testing design.

5. With wishes to the reviewer while showing deference, we have backed off the quote from Fields and Goran 2000, in fact we took it out in the introduction.

Methods

1. We chose density because it is more meaningful per se than simply reporting body volume (technically ADP does not directly measure body volume it estimates it based upon the pressure inside the chamber) Analyzing the %fat data did not change the message from the density data (a significant difference for the group and males, but no significant difference for the females).

2. Thank you for bringing this omission to our attention. We have added the appropriate reference for the
Bland-Altman in the methods with the correct description in the last paragraph.
3. We have added the appropriate citation for the TGV procedure and explained the procedure better and hopefully, clearer.

Results
1. We would respectfully disagree, the measured TGV is accurate and valid and is based upon the traditional respiratory TGV measurement using in a "plethysmograph box", which has been shown to be valid. As a result, we only compared the measured TGV.

Discussion
1. We removed the statement about HW and gold standard.
2. I am unsure of what the reviewer means.
3. We agree and have removed the whole statement concerning mineralization only affecting HW.
4. We noted that Dewit did measure RV simultaneously.
5. We agree and shortened the discussion paying careful attention to removing unnecessary speculation.

MINOR ESSENTIAL REVISIONS
We are unsure of specifically which labels are missing from which Figures or the wrong use of a term, can you please identify?