Author's response to reviews

Title: Application of Bioelectrical Impedance in large epidemiological studies

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

Re: Is Bioelectrical Impedance accurate for use in large epidemiological studies?

On behalf of my co-author, I would like to thank you for considering the above manuscript for publication in the Nutrition Journal. I would also like to thank the reviewer for very constructive feedback.

We have revised the manuscript based on the reviewer’s comments. A point-by-point response has been prepared for your consideration. Also, we changed the title of the manuscript to better fit the content. We appreciate the opportunity to improve the quality of the manuscript.

Sincerely,
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Reviewer's report
Title: Application of Bioelectrical Impedance in large epidemiological studies
Version: 2 Date: 30 April 2008
Reviewer: Duncan J Macfarlane

Reviewer's report:
This article provides an overview of how bioelectric impedance (BIA) can be used to estimate percent body fat in large epidemiological studies and the range of factors that can influence its measurement. Overall is it quite well written and easy to read, thereby providing readers new to this area with a good understanding of BIA and its many limitations. The standard of writing is certainly acceptable, but a few minor typo’s have been noted. Of more concern is the overall focus of the article and what it is trying to achieve, as the title, the text and conclusion are not fully consistent.
I feel this article does not provide any particular new in-sights into the area, which already contains a number of similar review papers (including some recent excellent book chapters), although few focus specifically on larger epidemiological studies and for this reason this article remains useful, if lacking focus. As the authors mention, this area has received a large number of publications over the last 10+ years, hence it is a difficult area to summarize, especially as the BIA technology has developed considerably since the 1990’s. To me the main problem I had in reading this review was that it lacked a bit of focus and contained a wide mixture of articles that refer back to early tetrapolar electrode systems that required the subject to be supine (with relatively long electrode preparation) and more modern foot (or hand)-to-foot systems that allow the subject to remain standing and take a few seconds to complete. The consequence of combining referencing to old and new technology was confusing and often sent a message that BIA was inconvenient. For example, on Page 6 the authors refer to crossing of legs and to the fluid shifts whilst being supine for up to 60 minutes. Almost none of this information is relevant to the most recent machines that allow the subject to stand for less than a minute to be measured. Furthermore, it could be argued that few researchers would even consider using the supine tetrapolar machines for “large epidemiological studies” (the title of the article) due to the greater time and inconvenience. Thus I feel the authors should have restricted their review to purely the most recent developments of BIA technology as this is more relevant and would provide the reader with a much more clear and less confusing understanding of how BIA technology can be used in large epidemiological studies. I therefore suggest the review be re-focussed on more recent BIA technology only to reduce the perceived confusion to new readers.
I feel the authors should have also mentioned the range of devices available (eg. hand-to-hand, foot-to-foot, hand-to-feet BIA devices etc) so as to provide better (review) advice to those considering using BIA in larger epidemiological surveys. However, not sufficient mention is made of the newer technologies (nor their validity/reliability) and the reader is not fully aware of what BIA devices can actually be applied to such surveys (as the title would suggest this might be included).

**Major compulsory revision:**
To *refocus the review on more recent BIA technologies.*
We have changed the focus of the review to the recently developed systems such as foot (or hand)-to-foot BIA.

To include more information on the range of BIA devices available (applicable to large surveys) and to provide more substantive evidence to support their validity/reliability for use in large epidemiological studies.
We agree with the reviewer’s comments and we have included some information about the validity of BIA in different conditions.

Furthermore, the current body of the article seems to focus more on limitations of BIA, which is important to consider but already well known, but it could be questioned if much of this can be controlled in large epidemiological surveys (eg. is it realistic to be able to control hydration level, exercise, medication, menstrual phase, etc in a large epidemiological study that often involves field testing – I would hazard to guess it is arguably not easily possible).

We thank the reviewer for the constructive comments and we have added some phrases in the abstract to make this point clearer.

The conclusions (especially the abstract) do not really reflect the main body of text (which focuses more on BIA limitations; more attention should be focused on recent validation/reliability studies if the authors wish to make such a conclusion and indeed I think this would change the review from being rather too similar to other reviews, to a more focussed and unique contribution that would be of considerable interest to readers).

This is a very valuable comment and we have added some more studies on the validation of BIA in the text to show the difficulty of employing BIA for large epidemiological studies with different ethnic groups.
Minor essential revisions:
Page 5 – last line. Mentions that the results have been inconsistent, but no evidence (refs) are used. Please support such a key statement with references.


Page 7. It is understandable to mention that errors in height and weight will influence %BF, but the examples state a 1 inch (2.5cm) and 1kg error respectively, but it is highly unlikely (even in a field setting) that such large errors are made? Are smaller errors linearly reduced? This statement is removed from the text.

Page 8. May need to reword by adding “In contrast, Chumlea et al …? ? The authors state Chumlea found no effect of food on BIA, but then on the next line summarize that “for these reasons an overnight fast is recommended”. The preceding sentence opposes this suggestion, hence needs “In contrast” to be added else it does not seem to follow logically.  Done

Page 8. No mention is made that the activity status of the person can be a critical factor in predicting %BF using BIA and some foot-to-foot machines allow an “athlete or non-athlete” input. The correct input here is critical as it can make a huge difference to the predicted %BF. Several authors have shown that using foot-to-foot BIA devices are sensitive to whether they are set to the “athlete” or “non-athlete” setting. Using such devices on large epidemiological surveys that are likely to include athletic sub-groups is therefore worth mentioning.

We thank reviewer for suggesting these studies and we have added some points from the suggested studies.

Page 11. Unclear where the “preceding hypothesis” is referring to. Please try to rephrase the sentence.
The statement is rewritten.

Page 12. To me the conclusions should not add any new data, but the several assumptions presented (a, b, c, d) are not fully covered by the previous text (especially the 16-34 BMI, which is somewhat confusing) – hence it is not clear how this compromises the BIA measure. Furthermore, it is concluded that BIA reasonably estimates body composition in controlled conditions (which is indeed true), but the main text of the article barely mentions empirical evidence that supports this conclusion and focuses more on factors that can comprise BIA measurements. But given the title refers to large epidemiological studies, it would seem perhaps more pertinent to actually review studies have indeed looked at BIA validity from field studies (and not controlled environments); this actually has not been done. Hence the title and conclusions are not in congruence with the main body of the text.

We changed the title of the manuscript to better fit the content.

Page 13. The well-known Tanita brand of foot-to-foot BIA devices certainly DOES have an Asian predictive equations for its machines, but I do not know if it has a Middle Eastern equation. I am not sure if other manufacturers have similar ethnically derived equations.

We could not find any developed equations for Arab and Iranian populations in the Middle East.

The references are adequate but focus perhaps too much on early work and not so much on the most recent studies (the authors themselves state over 200 studies in recent years) and perhaps several key chapters from books such as Heymsfield et al “Human Body Composition” and Heyward et al “Applied Body Composition” might be useful for a review that is likely to be read by non-specialists in this area as these books provide good background information on body composition in general.

We have learnt from the two suggested books and added some points to our manuscript.

A number of typo errors:
Page 4 Line 16 replace semicolon with a comma (after the 50kHz) Done
Page 5 Line 9: add “the” arms and legs Done
Page 5 last line. Separate “inconsistent The” Done
Page 6 Line 9: delete second occurrence of “factors”  Done
Page 7 Line 2: unclear what this second sentence is saying – please try to re-phrase.  Rewritten
Page 8 Line 13: results in; not resulting in reduced impedance  Done
Page 8 Line 21: change affect to affecting  Done
Page 10 Line 2: add – a loss in lean mass and a gain in weight  Done
Page 12 Table 2 line 2: weight mentioned twice – this should be “weight or height”  Done
References: 57, 59 & 60 have capitals incorrectly used.  Corrected