Author's response to reviews

Title: The Impact of Waist Circumference on Function and Physical Activity in Older Adults: Longitudinal Observational Data from the Osteoarthritis Initiative

Authors:

John A Batsis (john.batsis@gmail.com)
Alicia J Zbehlik (alicia.j.zbehlik@hitchcock.org)
Laura K Barre (lkb35@cornell.edu)
Todd A Mackenzie (todd.a.mackenzie@dartmouth.edu)
Stephen J Bartels (stephen.j.bartels@dartmouth.edu)

Version: 5 Date: 22 July 2014

Author's response to reviews: see over
Submission date: July 22, 2014

Dr Nagaraj Nagathihalli
The Nutrition Journal Editorial Team

Dear Dr. Nagathihalli:

We are pleased to submit our revision to our article and the responses to the reviewers are indicated below:

Editorial Comments:
Clearly state your data on WC in context and specifically how does it compare to BMI?. If you have prior published data on BMI, this should be clearly stated in the Abstract and Introduction.
The authors have made some additional statements noting what our WC results and not suggesting that it is superior/inferior to other measures. We have noted both in the abstract and in the text that the R² are similar.

Reviewer #2
I am not sure what to make of the authors’ response. The authors keep stating that central adiposity is an important (unique) predictor of the quality of life outcomes, but they DON’T SHOW IT with their data. They simply substitute WC for the BMI and find that WC is as good a predictor of quality of life outcomes as the BMI. To say that “not considering WC may ignore a considerable sample of subjects at otherwise risk of adverse outcomes” is a CONJECTURE, but not a conclusion based on the EVIDENCE PRESENTED HERE. In the manuscript (p.14), the authors claim: “It is likely that abdominal adiposity places a disproportionate burden on the musculoskeletal system that limits overall mobility, and have independently confirmed its association with physical limitations in subjects with normal BMI [Batsis submitted].” If you have confirmed this in another (not yet accepted/published) paper that you claim overlaps with this one, what is the contribution of THIS paper? Again, just substituting the WC measure for the BMI and not finding it to be a better predictor does not warrant your conclusion that targeting patients with high WC, instead of high BMI, yields an extra pay-off.

We fully acknowledge Dr. Stommel’s comments and recognize that in the epidemiological literature, there are advocates for BMI and advocates for other anthropometric measures (including WC). The research literature fully addresses the shortcomings and advantages of utilizing both approaches.

Our intent in analyzing this dataset using WC was relatively simple and straightforward – can this anthropometric metric predict disability and quality of life in a longitudinal manner. We neither intended on proving that this metric was superior (or inferior) to BMI, and neither could we account for BMI in our analysis due to the high correlation (r=0.79) and thus high collinearity in our multivariable modeling.
We have added in our manuscript additional information outlining the above due to the shortcomings observed with age of BMI in the literature. We do believe that our data indeed proves the relationship between WC and QOL and disability and is unique in that we are examining this modeling in a population at risk for osteoarthritis.

We have eliminated the statement pointed out by Dr. Stommel, “It is likely….”. We recognize that this was not proved in our manuscript (but based on previous work using other datasets). We wrote a statement that identifying subjects with normal BMI and high WC (or vice-versa) is outside of this particular study, and can conceivably be performed in the future.

Additionally, we agree that the statement above is based on conjecture and not proof. We specifically alluded to our previous work noting the importance of WC as an alternative measure and that a next step would be to perform future analyses using this cohort combining these metrics.

The authors have made an effort to ensure that our message is consistent throughout the manuscript that we are not attempting to prove that WC is superior or not. On page #17, we have changed the implications of the R² to read as follows which we believe ‘tones down’ the implications our findings:

While the R² in our modeling did not differ greatly (Appendix #1), using WC may still be an important anthropometric variable to measure. Not considering WC as an alternative measure may ignore a considerable sample of subjects at otherwise risk of adverse outcomes as we previously have demonstrated[32] suggesting the need for future analyses to determine the impact of these combined metrics on outcomes.

We hope that the above explanation is satisfactory to the editor and to the reviewer and happy to make any specific changes requested.

As to the point about the non-linear relationship between physical activity and the continuous WC, I don’t agree with the statement that the “clinical interpretation of polynomial regression analysis is a rather difficult process and would be markedly compromised (?).” However, I am willing to give the authors the benefit of the doubt here. We thank the reviewer for their comment on this point.

We appreciate your consideration of this manuscript. Should you have any queries regarding this submission, please do not hesitate to contact me at john.batsis@gmail.com for any questions or concerns. Again, thank you for your consideration.

Sincerely,

John A. Batsis, MD, FACP
Assistant Professor of Medicine
Geisel School of Medicine at Dartmouth
Dartmouth-Hitchcock Medical Center
1 Medical Center Drive