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

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Version: 3
Date: 5 June 2014

Author's response to reviews: see over
Manuscript ID#: MS: 212855188122647

June 5th, 2014

Dear Dr. Nagathihalli:

I would like to thank you and the reviewers for your critical review of our manuscript titled, The Impact of Waist Circumference on Function and Physical Activity in Older Adults: Longitudinal Observational Data from the Osteoarthritis Initiative. We have attempted to address all the concerns and have edited the manuscript accordingly. Comments were addressed as follows:

Reviewer #1:
We would like to thank Dr. Stommel’s input and suggestions. The authors have addressed each point as indicated below:

Major Compulsory Revisions:
(1) The authors state that “Our previous work suggested that BMI in older adults suboptimally identifies adiposity. We have argued that other anthropometric measures, either in lieu of BMI or in addition to BMI should be used in older adults (Batsis et al Under Review J General Internal Med).” I do not dispute that the BMI “suboptimally identifies adiposity,” but the question remains: “so what?” Only if knowing a person’s adiposity improves our ability to predict outcomes (such as the quality of life measures used here), would it makes sense to use a separate measure of adiposity in addition to, or instead of, the widely used BMI. Thus, the issue is not whether the BMI is a good measure of predicting adiposity, but whether the adiposity measure adds anything to our knowledge and predictability of quality of life measures. You state yourself that “we previously demonstrated in a similar population that BMI impacts quality of life and physical function[Batsis et al Under Review, Public Health Nutrition], and this current study suggests that abdominal obesity could be a separate predictor of poor functional outcomes. Future analyses could determine the impact of the combined or independent use of these metrics, in addition to evaluating the incremental predictive nature of these variables on long-term outcomes.” Since you have the data, why wait for ‘future analyses”? Why can’t you find out and state (in the discussion section) whether using the BMI or WC yields better predictability of the quality of life measures? For instance, if you ran the same regression models, except for using the BMI instead of the WC measure, which one would yield a better R-squared?

The authors concur with the reviewer’s comments. As part of this revision, we have compared the R² values for each model using BMI and WC as separate primary predictors. We are also mindful of the concern for duplicate publication – that is, if we were to publish the results of the BMI analysis in this current manuscript, we would be in violation of the terms and conditions put forth in the submission to Public Health Nutrition (manuscript which is still under review and
was submitted prior to this current manuscript). As such, we have inserted a statement noting the comparison of R-squared as requested by the reviewer (page 16).

“While the R² in our modeling did not differ greatly between using WC quartiles and BMI standard categories (Appendix #1), using WC is important particularly in older adults with normal BMI who may otherwise only have central adiposity, and thereby miss an opportunity for prevention of distal outcomes [Batsis et al AJC, Eur J Intern Med]. Not considering WC may ignore a considerable sample of subjects at otherwise risk of adverse outcomes.”

As is observed, the R² is marginally lower in the BMI analysis than in the WC analysis. We have also altered the statement noting (p. 17):

“Future analyses could determine the impact of these combined with normal BMI and central adiposity or independent use of these metrics.”

Additionally, we have inserted a table comparing the R-squared for the Waist Circumference and BMI models solely for the results in Appendix #1 (ie: comparing R² for modified vs. regular Charlson score), and referenced the submitted article.

We trust that this is satisfactory to the reviewer. The authors would be more than willing to consider any alternatives to this dilemma in an effort to avoid publishing results that would otherwise be considered to have been submitted elsewhere.

(2) Appropos the regression models with the continuous WC predictor: You only introduce the WC variable as a linear predictor, even though the results from the quartile analysis clearly indicate that the relationship is often not linear: for example: for the 60-70 group, the physical SF sub-score means for each quartile in Table 3 are: 45.9, 47.8, 46.2, 45.9. This suggests an inverted U-shaped relationship, which would require a polynomial regression of the 2nd degree. Several of the other mean outcomes for the four quartiles suggest such relationships. Thus, having the effect of WC presented by a single coefficient for the linear WC term amounts to mis-specifying the regression model.

The authors recognize that this may be an issue. In observing the adjusted values, many of the confidence intervals do overlap with each other, suggesting there are minimal if any differences between successive categories.

We agree that mathematically, we could conceivably utilize a polynomial regression model should we introduce WC as a continuous variable, and this possibly could have been an error on our part in our previous response to reviewers. In consultation with our statistician, Dr. Mackenzie, the authors agree that it always is a balance between introducing such terms and the interpretability of these terms. As could be appreciated, clinical interpretation of polynomial regression analysis is a rather difficult process and would be markedly compromised, and hence another reason why we elected to categorize by categories, to obviate the need to perform such complex analyses that otherwise may not provide significantly more information.

While we agree that adding complex polynomial regression analyses (2nd or more degree) would lead to more accurate representation of the regression modeling, we have elected to eliminate the
Minor Essential Revisions:

(1) I am not fully persuaded by your arguments concerning the use of the Charlson Index as a control variable. This index is made up of several items indicating the presence of heart disease and also diabetes, for all of which central adiposity is a known risk factor. Thus, ‘controlling for’ this index amounts to partially controlling for an outcome associated with central adiposity. Since you present the unadjusted associations between the outcome measures and the WC quartiles, this is acceptable, but a better alternative would be to use a modified Charlson Index that removes the indicators that are potential outcomes of adiposity.

We appreciate the comments raised by Dr. Stommel. We identified the following indicator variables of the Charlson co-morbidity index that are potential outcomes of adiposity and have eliminated them from the analysis: heart attack, heart failure, peripheral vascular disease, stroke, asthma, diabetes, liver cirrhosis (unfortunately cannot identify the cause in this dataset and since non-alcoholic fatty liver is a common cause, we elected to eliminate it), and cancer (since it itself is associated with obesity, central adiposity and disability). Notably, the other patient characteristics were absent from this population. The remaining variables were adjusted for: emphysema, stomach ulcers, renal disorders, kidney transplant, rheumatoid arthritis, cancer.

The authors weighted the variables using the original index to create a modified Charlson score. We believe that the variables omitted can be justified for reasons outside the scope of this manuscript but related to the quality of life, disability and obesity. The current version of the manuscript reflects the values from this index. We elected present data with the original Charlson co-morbid index score (with all of the variables) in an Appendix for the reader for their interest.

Generally, the adjusted mean values of all outcome variables are higher using the modified index, while the adjusted mean differences are slightly lower. The statistical comparisons parallel those of the original analysis.

The statistical analysis section and methods section were also updated to reflect this. Notably all patients were free of cognitive impairment at baseline.

(2) Some sentences in the Discussion section got mangled:
“Lastly, our main predictor was using WC quartiles in line with previous author’s arguments in that it allows study (?) to study comparison of anthropometric indices[23].”

We apologize for this. The authors have rewritten this as follows:
“Lastly, using WC quartiles as our main predictor was in line with previous author’s recommendations in that it facilitates comparison of anthropometric indices between studies.”
“Although our data was consistent with theirs, although (?), these authors did not specifically look at older adults, nor did they utilize PASE, SF-12 or LLDI as primary outcomes.”

The authors deleted the word ‘although’ from this phrase

(3) This sentence in the limitations section lacks specificity: “We agree that certain variables could conceivably have been omitted and/or excluded, and that others could have been considered in our analysis.”

The authors clarified this phrase by rewriting it as follows:

“We agree that certain variables, including social support, depression, and physical measures could conceivably have been omitted and/or excluded from our analysis.”

Additionally, since certain variables in the Charlson co-morbidity index are associated both with central adiposity and our outcomes, we created a separate index and adjusted for this, and found that our results paralleled the original analysis.

“To account for possible confounding between predictor variables in the Charlson index, we restricted our modified index to those factors likely not influenced by obesity or disability outcomes. Although the estimates were slightly different, trends were not.”

We hope that the Reviewers and the Editorial team are satisfied with the above clarifications and responses of the concerns brought forth. We hope that this revised manuscript merits publication in Nutrition Journal and we would welcome any further comments or criticisms that could improve the quality of this manuscript, in an effort to publish this work. Please do not hesitate to contact me at john.batsis@gmail.com should you have any questions or concerns.

Sincerely yours,

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