Author’s response to reviews

Title: Multi-parametric Muscle and Fat Correlation of Computed Tomography Parameters to Outcomes in a Total Hip Arthroplasty Population

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1. The study employs CT to examine and quantify soft tissue parameters. While I understand CT has been historically popular, more advanced imaging methods (like DIXON MRI) are now preferred, particularly when examining muscle tissue composition. Indeed, CT is now very difficult to get past ethics approvals due to its high ionising radiation exposure. The authors have not justified why CT was their imaging of choice, which warrants explanation, and should additionally be indicated in limitations.

Response: Thank for the query. As this was a retrospective investigation, only CTs were available for those patients with hip scoring uniformly performed prospectively. Additionally, the Aquarius software works better in segmentation using CT data. Although MRI can provide fat fraction analysis with recent advanced techniques, it is a more expensive study and the imaging may be limited by metal artifact. We have added this explanation to the discussion (Discussion, Page 10, Lines 12-34).
2. The study is a retrospective analysis of a small sample; both of these render the study as low level evidence. I appreciate Fig2 in partly describing why the sample was so small, but the exclusions should be detailed further; e.g. If they didn't have CT imaging, what did they have? Perhaps the biggest problem with such a small sample is the inability to sub-group analyse the data, and there are variables like sex that are known confounders to muscle composition and would benefit from separate analysis. I presume this is a single register study, and I wonder if the authors have considered teaming up with other sites in order to provide a more powerful sample?

Response: The reviewers point is well taken, but this study serves as the initial exploration of this subject. Ideally, our results will encourage a large, multi-center trial on this subject with more prospective evaluation. We wanted to include the patients with both a complete CT data set and those who had complete sets of HHS and WOMAC scores, which limited our sample size. But, a sensitivity analysis was performed and the study group was not markedly different from the overall registry group, aside from patient heights being slightly different (Table 1). This has also been expanded upon in the discussion (Discussion, Page 9 Line 45 – Page 10 Line 12).

3. CT assessment involved bilateral single (3mm) slices for each of psoas (assume major, but this is not described) and gluteus medius-minimus. While location of the slice has been loosely anatomically described, how you have defined the regions of interest manually is not described in text, and this is very difficult to identify in Fig1, which does not have anatomical markers or a legend to direct the reader. At the very least, there should be some anatomical reference to inform the reader so they can make a judgement as to how your methods compare with others; there is a weight of literature for both psoas and gluteals (separately) in methods used to define ROI. Also, muscle proponents now favour a multi-slice (and even whole volume) approach to examining muscle composition, as a single slice may not be representative of the entire muscle. The authors should include these points in their limitations, and clarify the text.

Response: We have further detailed the definitions of our slices of interest (Methods, CT Assessment, Page 5, Lines 36-44). As noted, multi-slice evaluation is now available with recent software upgrade and we will use it in the future study. We have mentioned this in our discussion of limitations (Discussion, Page 10, Lines 36-44).
4. You have indicated that two readers have collected the measurements, at first together 'in training' and then independently; it is unclear whether both readers have measured ALL cases, or if the dataset is pooled from each? You have provided ICCs for inter-rater reliability of the method, but not intra-rater, and there does not appear to be any information regarding the professional background or experience of the readers. If available, both intra- and inter-reliability should be reported. Please consider that you are reporting methods in order for a reader to draw comparisons and/or repeat your methods in their own dataset; your description of methods is limited.

Response: We have further detailed the experience of the readers and radiologist (Methods, CT Assessment, Page 5, Lines 28-36). We have also added to the discussion future prospective exploration with intra-reader performance (Discussion, Page 9, Lines 45-51).

5. The discussion misses key literature surrounding quantification of hip muscle composition, either technically, or the hip region itself; whether a THA sample or not, other papers should be discussed in relation to your findings; the works of Alison Grimaldi (Australia) and Magda Marcon (Switzerland) might be a good start. In doing this you will also better indicate the clinical relevance of your findings, which are presently not that clear.

Response: The discussion has been expanded with reference to recent literature on use of MRI determination of muscle and fat composition (Discussion, Page 10, Lines 26-37). Thank you for the suggestion.