Reviewer's report

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

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Reviewer: Rebecca Crawford

Reviewer's report:

The authors should be encouraged for endeavouring to make a novel contribution to the literature in using imaging parameters in a total hip arthroplasty population; however, I am struggling to see the value-add to the literature, and the paper may benefit from consideration of the following comments.

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. I am uncomfortable supporting studies that employ these legacy methods, unless there are substantial retrospective sample sizes to warrant the use. The authors have not justified why CT was their imaging of choice, which warrants explanation, and should additionally be indicated in limitations.

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?

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.

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, which is an unfortunate omission, and there does not appear to be any information regarding the professional background or experience of the readers, which can markedly influence the repeatability of results. Intra- and inter-reliability should BOTH be reported, and I would content that three CTs does not represent sufficient power for reliability analysis. 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.

5. I am surprised with the length and content of your discussion. It does not discuss 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 clear.

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

No

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

No

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.

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