Reviewer's report

Title: Rapid T1 Quantification based on 3D Phase Sensitive Inversion Recovery

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Reviewer: Scott King

Reviewer's report:

General Comments that I feel will make this a good paper.

My main critique with this manuscript is that the authors did not demonstrate very well a strong clinical need for this quantitative T1 mapping technique. That is, they didn’t do a good job “selling” their method. Common thought is that the relative T1 information obtained between healthy and fibrous tissue is fine on its own and quantitative information is not needed, especially if getting this quantitative information leads to other negative effects such as longer scan times or longer reconstruction times. Therefore the authors have to sell the clinical need for their method. Having said that, I do feel there is a clinical need for their method, and this needs to be more strongly brought out of the paper. It starts with the title which I feel needs to reflect the need for this method. The authors identify the need to a quantitative T1 mapping method in the background as being able to monitor the actual dynamics as opposed to relative T1, and that this is needed for improved stability of segmentation, which was not supported (references) in the manuscript, and secondly for follow up studies. I think this second point has validity, but it was not strongly supported. Finally, a third need of the this quantitative method is identified, but not until the discussion section on pg 14 5th line, as this method is insensitive to RF coil sensitivity. In the end it was this feature that “sold” the method to me.

The other main general comment is that the title seems to highlight a rapid 3D method, yet the breath-hold time is still 24 sec (too long for most patients). Yet, to me, since the method is for measuring T1 information during the “late enhancement” stage where there is plenty of time to image, why was a 3D method needed. A multiple 2D method may have been better since breath-hold times could then be lowered. The authors need to justify why the 3D method was used.

1) Title should reflect the need or advantage of this method. I suggest something like “Quantitative T1 Mapping Insensitive to RF Receive Coil Inhomogeneity using rapid 3D Phase Sensitive Inversion Recovery”

2) Background section, second paragraph is not strong. The authors really need to convince the reader that a quantitative T1 mapping is clinically useful.

2a) Move the statement “A variety of T1 mapping methods exist (see refs 11-15)” to right after the first line of the paragraph.

2b) First, if the authors could then make some statements about these existing methods that tell the reader that they do not allow the clinician to segment well
and perform follow up studies on a different day with possibly different scanner settings. This would then justify the next statements the author has made as advantages of a quantitative method.

2c) Second, the authors need to make some statements about why a 3D method is important since it appears to me that a 24 second breath-hold is too long for most patients. Because to me, since the method is for measuring T1 information during the “late enhancement” stage where there is plenty of time to image, a multiple 2D method may have been better since breath-hold times could then be lowered. The authors need to justify why the 3D method was used.

2d) Thirdly, if it is true, the authors also need to state in this background section that existing methods are sensitive to inhomogeneity of receive coil sensitivity, and how this is a problem for accurate diagnosis which to me is a real problem, depending on the severity of the inhomogeneity, and could be a main advantage of this method.

2e) Finally, follow up these statements stating that you have developed a rapid 3D quantitative T1 mapping method that is insensitive to receive coil inhomogeneity, which allows accurate segmentation of healthy myocardium and scar tissue even during follow up studies using different scanner settings.

In summary of my comments regarding the background section, state all of the clinically relevant problems with the existing methods and then state that you have a new method that solves these problems!

3) page 11: The first mention of Fig.5 should describe what A-F are (healthy, fibrotic, fat etc)

4) page 11: the statement “The relaxation rate of complete myocardium is estimated ...” is confusing. What does “complete” mean? Maybe a different wording could be used here.

5) page 11: Again, to some people, the fact that the dynamic curves look the same except for a shift in R1, between healthy and fibrotic tissue suggests that the dynamic information is not important but simply one data point would suffice, say at 15 minutes. The authors could comment on this.

6) page 11: “According to Fig.6 hyper-enhancement of fibrotic tissue is already present after 5 minutes and slowly increases during the following 30 minutes”. Are you drawing attention to the difference between healthy and fibrotic tissue or the fibrotic tissue alone? Because a similar statement could be made regarding the healthy tissue. Please re-phrase.

7) page 14: The statement “Actins to minimize …, might decrease the image quality” is poorly stated. These are two very important points that should be made separately. The authors could state that without a quantitative T1 method, fat suppression techniques may need to be employed and then state a few negative effects of having to include fat suppression. This also might be added as a “need” in the background section!

A separate statement should be used about having to use a uniform receive coil sensitivity such as a body coil, that would mean lower SNR and thus lower quality images; but this should be stated right after the statements regarding RF
coil sensitivity.

8) The conclusions are weak. The authors could strengthen the conclusions by re-stating that a quantitative T1 mapping method was developed that allows accurate segmentation during follow up studies, that is insensitive to the receive coil inhomogeneity and therefore, phased array coils with high SNR can be employed, and does not require fat suppression for correct interpretation of infarct area.

Minor Essential Revisions:

1) page 6: Equation 8, I think “expTC” should be replaced with “exp(TC)”

2) page 7: “Prerequisite, however, …” should be changed to “A prerequisite, however, …”

3) page 14: remove the word “resulting” from the statement “The resulting bright fat signal in the LGE images may lead to an incorrect interpretation of the size of the infarct area”, as this suggests that the bright fat signal is a result of the previous statement on coil sensitivity, which I don’t think is the intention.

4) page 15: “The ability to synthetically vary $T_{inv}$ after the actual may …” is confusing. After the actual what? Please re-word.

5) Page 17: Reference #7 is incorrect. The journal is Magn Reson Med

Discretionary Revisions:

1) page 5, Explain why “A low k-space profile order ensures that the image intensity is not altered due to differences in MB …”

2) page 10: It might be useful to the reader if you comment on why the error bars are larger for longer T1.

3) Page 10: Please site a reference or explain the particular use of the Monte Carlo simulation

4) Fig.5: The boxes are hard to see in the color figure, but ok in the grayscale version when printed.

5) Fig. 6,7: It would have been really nice to see time zero prior to gadolinium on the graph.

6) Fig.5-7: Generally I got confused looking at Fig.5 which is a T1 map, and then looking at Fig.6,7 which are reciprocal R1 maps. The authors could include a R1 map in addition to the T1 map.

7) page 12: Explain why “the difference in standard deviation which the LL method is 2-3 times that of the proposed methods”, it is not clear why this is the case.

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable
Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:
I declare that I have no competing interests.