Author's response to reviews

Title: Monte Carlo-based Noise Compensation in Coil Intensity Corrected Endorectal MRI

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Author's response to reviews: see over
Dear Editor and Reviewers,

The authors of “Monte Carlo-based Noise Compensation in Coil Intensity Corrected Endorectal MRI” would like to thank the editor and reviewers for their highly valuable comments, as it has led to a significantly improved revised manuscript. We have made significant efforts to address the concerns and comments of the reviewers and have identified them individually below. We hope the revised manuscript will be found acceptable for publication, and we are more than willing to make additional changes if necessary. The reviewer comments are highlighted in *italics* while the authors’ responses are highlighted in **bold**.

*Editorial comments:*

- The manuscript was assessed by an expert in the field and myself. Both reviewer think that your manuscript is interesting and mostly well written. However also a lot of points to be addressed were raise so that at present the manuscript cannot be accepted for publication. Therefore I ask you to perform a major revision of your work addressing all points by the reviewers. Please provide your comments in a rebuttal letter with the resubmission.

We thank the reviewers for finding the manuscript interesting and mostly well written. We have tried our best to address the points raised and hope the revised manuscript is found acceptable for publication.

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*Editorial request:*

1.) Ethics

**Kindly include the full name of IRB.**

- Please update your ethics statement to include the name of the ethics committee that approved your study.

We have now included this information in the revised manuscript.

2.) Kindly include email address of co-authors

**Authors contacts**

We have now included this information in the revised manuscript.

- Please include a title page at the front of your manuscript file. It should contain, at minimum, the names, institutions, countries and email addresses of all authors, and the full postal address of the submitting author.

We have now included the cover letter in the revised manuscript.

*Reviewers’ comments:*

**Reviewer 1:**

The authors present a noise reduction algorithm for images derived by using endorectal coils during the MRI. The algorithm is compared to three other techniques. Overall the manuscripts is well written but lacks some details.
We thank the reviewer for finding the manuscript to be well written and we have tried to add the details requested by the reviewer. We hope the revised manuscript is acceptable in its revised form.

1. The description of the MR imaging is insufficient and some more details about the image recording is needed. Especially since the MR images look pretty distorted and are overlaid by pulsation or ghosting artefacts.

We thank the reviewer for pointing out the need for more details and we have now included 3 tables in the revised manuscript depicting the data collection protocols (and associated imaging parameters) for each of the experiments performed in the study. We have also explained in the revised manuscript that the data collection protocols are standard clinical data collection protocols used at Sunnybrook Health Sciences Centre and so are very representative of the quality of images captured in a current clinical scenario (these images were used for clinical diagnosis and screening at Sunnybrook Health Sciences Centre), which motivates the development of noise compensation algorithms as those clinical protocols are already optimized for best image quality already for clinical use. Also note that the aforementioned distortions and ghosting artefacts are not exhibit in the prostate gland region, which is the only important region for the diagnosis of prostate cancer (the other regions are ignored and not used for clinical assessment).

2. All tables miss std deviations with the average values, please add.

We thank the reviewer for pointing this out and we have now included std deviations with the average values for all necessary tables in the revised manuscript.

3. I am not sure whether a endorectal coil plus computational intensive noise correction or imaging with more averages would achieve the same results. Please comment.

We thank the reviewer for bringing up this point. One approach to addressing the noise issue is indeed to perform multiple acquisitions and average over the acquired data. However, such an approach is very prohibiting and not feasible in a clinical setting given that the imaging time increases significantly to perform multiple acquisitions, which reduces the number of patients that can be scanned, as well as the fact that such an approach increases the possibility of motion artifacts due to patient motion during long imaging sessions. We have now commented on this in the revised manuscript.

Reviewer 2:

The manuscript describes a computational approach that enhances Signal/Noise (SNR) in Magnetic Resonance (MR) images when acquired with endorectal loop coils (ERC). The authors report an average improvement of background SNR to be 11.7dB and prostate SNR to be 11.2dB. The results were compared to other computational approaches. This new approach takes into account the highly characteristic SNR profile of the ERC. The manuscript is very well written.

We thank the reviewer for finding the manuscript to be very well written, and we have tried our best to address the comments made where possible and hope the revised manuscript is considered acceptable for publication in its revised form.

1. The authors decided to measure SNR as the average across a ROI divided by the standard deviation of this ROI. While this approach may be suitable for other image processing applications, SNR estimation from MRI images is best computed for each single voxel location and a multitude of repeated measurements especially when the sensitivity of the detector varies across the large chosen ROI as used in this study. As concluded by Dietrich et al. “As soon as reconstruction filters or techniques such as parallel imaging influence the spatial noise distribution, only those methods of SNR measurement remain valid that determine the noise at the same spatial position as the signal.” [Dietrich et al., Measurement of Signal-to-Noise Ratios in MR Images: Influence of Multichannel Coils, Parallel Imaging, and Reconstruction Filters JOURNAL OF MAGNETIC RESONANCE IMAGING 26:375–385 (2007)]. I am aware that such measurements are difficult to obtain from participants, but repeated measurements
would certainly be possible using the presented phantom. It is necessary to present in a figure a spatial distribution of the spatial noise distribution in relation to the used ERC.

We thank the reviewer for bringing up this point. The method of measuring SNR that we have used was chosen for this study based on the fact that the use of ROI for measuring SNR is the most widely used approach for practical, clinical quality assurance [1,2,3,4,5], and the use of ROI is the approach standardized by the National Electrical Manufacturing Association (NEMA) [4] as well as by the American College of Radiology (ACR) [5]. We have now included citations for the ROI approach of measuring SNR in the revised manuscript to improve justification for such an approach. While we do appreciate that the measurement of SNR based on multiple acquisitions has merit and we really appreciate the reviewer’s comment, it is very rarely if ever done in clinical practice, and especially never done for prostate MRI in clinical practice, because it has been shown in studies that SNR measurements based on multiple acquisitions require a large number of image acquisitions on the order of hundreds for whole-body MRI systems and, while very interesting, we do not have to resources to perform such a study at this point in time and so we have now discussed this in the revised manuscript as future work as part of a future study for a more thorough assessment of image quality. We hope the reviewer understands our situation at this point in time in terms of the inability to collect hundreds of acquisitions to perform the suggested SNR calculations, and also finds the current SNR measurement used since it is the most appreciated approach used internationally for MRI scanners.


2. Two of the presented comparison algorithms also enhanced SNR, but traded of spatial resolution. Although this is qualitatively visible in the presented Figure 5, it is advisable to rather use a resolution phantom in order to quantify the loss of resolution due to post-processing and spatial averaging.

We thank the reviewer for bringing up this point. While we would love to obtain experiments, we do not currently have access to a resolution phantom that we can use to perform this study and is beyond the scope of this study at this time. As such, we have now discussed in the revised manuscript that as future work it would be of great interest to perform a more comprehensive study in the effects of noise compensation on resolution. Furthermore, we have included quantitative assessment of edge degradation in the form of the widely-used edge preservation (EP) metric, which provides insight and quantitative assessment of loss of resolution (e.g., the better edges are preserved, the lower the resolution loss).

3. The authors compared whether the improvement of the SNR was statistically significant compared to the SNR in the uncorrected images. While this is interesting, a statistical test of whether the improvement of the suggested algorithm is indeed better than that of the other algorithms is missing. From the results table one may get the impression that all algorithms boost SNR nearly equally well – the difference between the SNR boost for each algorithm is relatively small (~1dB) compared to the overall improvement relative to the uncorrected SNR (~11dB).
We thank the reviewer for bringing up this point. We completely agree and we have now replaced that table with a table (See Table 7) showing statistical test of whether the improvement of the suggested algorithm is better than the other algorithms as well as compared to the uncorrected images, which should give much better insight as suggested by the reviewer.

4. The introduction makes a weak case for ERC coils. If the SNR improvement due to the coil is only factor 2 compared to phased array coils (the authors wrote: “For lower field systems operating at 1.5 T, an ERC is helpful in achieving performance similar to 3 T MRI with pelvic phased-array coils (PAC)”), why bother using endorectal probes that are difficult to position, uncomfortable for the patient, and time consuming to put in place? One may just use a twice larger voxel – instead of (3x3x3)mm3 with ERC the voxel size only increases to (3.8x3.8x3.8)mm3 - and arrive at a twice greater SNR with an externally-placed phased array coil and without the need for complex post processing algorithms as the one suggested herein.

We thank the reviewer for bringing up this point. We apologize for the improper wording of our statement in terms of the justification for ERC coils. The sentence has now been changed in the revised manuscript to “For lower field systems operating at 1.5 T, an ERC is helpful in achieving significant improved SNR (~10-fold) [6] and performance that is superior to 3 T MRI with phased-array coils (PAC) [7].” We have now specified explicitly that the use of ERC leads to much higher SNR improvement (a factor of ~10) that can be achieved even with 3T, and clinically shown in studies to provided improved performance when compared to 3T with phase-array coils.