Reviewer's report

Title: Acoustically detected Magnetic Particle Imaging

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Reviewer: Shelton Caruthers

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The authors present a nice, concise manuscript reporting theoretical and experimental results from a new method of detecting signal from magnetic particle imaging, i.e., acoustic detection. Magnetic particle imaging itself is a relatively new imaging technique which this group first published in 2005. In general, the theory, methods, results, discussions and conclusions are well defined and presented. However, since the authors suggest the ultimate application of this technique is for medical imaging, a more complete discussion of the limitations of the technique (and this experiment to characterize the technique for these intended applications) could be warranted. The following specific comments for clarification and/or improvement are suggested.

1. Introduction, 1st paragraph: As some readers may not carry the definition of “MPI” from the abstract, please repeat its definition following the first use of “Magnetic Particle Imaging” in the body text.

2. Please add a reference citation for “clinically approved contrast agent for liver examinations....”

3. In the 2nd paragraph, “FFP” is defined a second time.

4. Figure 1 and related text: The text in the body and the figure legend is confusing with regards to the direction of M and F vectors. The figure is drawn with magnetization vectors pointing towards the FFP, however, the legend claims M always points away from FFP. This section (and figure) is a very important, indeed critical, section of the paper as it describes the phenomena being generated/detected. In general, the description in the legend is a little more clear than the abridged text in the body.

5. P. 3: Following equation 1, the adjective “well known” is extraneous and, depending on the reader, potentially incorrect!

6. Following Eqn. 3: In describing the terms, when the authors say “…B being the magnitude of the field in x-direction”, do they mean that “B” is always defined only in the x-direction, or do they mean to have used Bx (B sub x) (i.e., a typo).

7. P. 3,4 last,first sentence: Where the authors claim that the resolution in acoustically-detected is 2x that achievable with magnetic detection, I believe it is more clear to re-iterate that the theoretical limit is 2x that for magnetic detection. The assumptions, limitations and artifacts for each technique may make one technique better at approaching nearer its theoretic limit (such details could also be incorporated into discussion section).
8. Figure 2: This figure is not referenced in the body text. Please add the reference and some explanatory text.

9. Figure 2 Legend: The phrase “It can be observed, that” in the legend text is extraneous (and incorrectly punctuated).

10. P. 4 last paragraph. The equation for P_N is not numbered.

11. P. 4 last paragraph. In calculating P_N, the authors state that R(Z) can be approximated as the product of density and speed of sound, assuming damping is negligible. Is this an appropriate assumption (negligible damping) in this case? What about the case of clinical imaging? (i.e., what is “negligible”?).

12. Figure 3 (legend): Should the word “infinite” should be “infinitely”, i.e., describing a point source?

13. Methods: Please add the particle size and distribution when first presenting the material used in Methods. The authors later imply that “most particles seem to be...” < 10nm. But the theoretical results are plotted (and compared) to particle sizes of 15 – 21 nm. Please comment further on the importance of this (or reference discussions in previous publications).

14. P. 5: “an diameter” should be “a diameter”.

15. P. 6 (and others). Occasionally, the authors use the spelling of “therefor” for “therefore”.

16. Results and Discussion: The authors state that data are in good agreement. Please be more specific. By what criteria do they agree, or within what limits? In some graphs, e.g., Fig. 6, to the casual observer, the experimental data might appear to have an error of 100% over theoretical.

17. Results and Discussion: This section could benefit from more discussion on limitations (as previously commented): limitations of this experiment and limitations toward use in medical applications (in vivo). E.g., in the theoretical calculations, 1 sec sampling was used for S/N calculations (0.5 sec * 10 samples used in experiments), is this the expected clinical implementation? (Likely it is not, so what are the consequences.) This is alluded to in the “outlook” section, but more could be included.

Level of interest: An article of importance in its field

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 previously (within previous 12 months of this review, but not currently), I was an employee of Philips Healthcare, whose parent company Royal Philips Electronics also owns Philips Research.