Reviewer’s report

Title: Contribution of diffusion-weighted imaging to conventional MRI for detection of haemorrhagic infarction in ovary torsion

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Reviewer: Rossano Girometti

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POINTS OF STRENGTH

1. The study deals with an interesting topic.

2. Images are nice.

However, I had the impression this study should be revised to sound convincing, as I am detailing below.

GENERAL POINTS OF WEAKNESS

1. The study background at page 4, lines 9-26. The Author cited just one previous paper (reference # 19) to support their study, without explaining why exactly DWI is expected to add relevant clinical information to conventional MRI. Moreover, the study hypothesis is somewhat confusingly expressed: which is the clinical relevance of distinguishing between haemorrhagic and non-haemorrhagic infarction? Change in management?

2. Methods, paragraph on "Imaging protocols". The description of DWI is quite generic for a study claiming to use it for clinical purposes. A) Which fat saturation approach was used (SPAIR?)? B) What "without breath-holding" means? Respiratory-triggered? Free-breathing? C) A 6 mm gap (greater than slice thickness = 5 mm) sounds quite surprising. D) Which PI algorithm was used? E) What about NEX? EPI factor? Bandwidth?

3. I had the impression that the Discussion is too long and confusing, failing in explaining which is the originality of the study content compared to studies # 19 and 20, and which is the real clinical relevance of the Authors results. In other words: which is the real expected clinical advantage or change in management due to the distinction between haemorrhagic and non-haemorrhagic infarction?

4. Please, revise English language, especially in the Background and Discussion.
SPECIFIC REMARKS

1. Background. I had the impression this section would benefit from re-organization. In particular: a) the content on page 3, lines 8-21, is introductory in nature, and should be moved at the beginning of the Introduction; b) statements at lines 3-8 and 21-32 are somewhat similar/redundant, and should be more synthetically presented.

2. Background, page 3, lines 47-60, and page 4, lines 1-6. There is too much description of CT and MRI findings here, in my opinion. The most appropriate place for such a detail is probably the Discussion rather than Introduction, in which you are expected to present the rationale for the study in a couple of synthetic key-messages.

3. Background, page 4, line 13. Please, define "semiquantitative". This sounds as an inappropriate term if you refer to qualitative signal analysis or quantitative ADC determination.

4. Methods, paragraph on "Patient selection and inclusion criteria". Though Fig. 1 is self-explanatory, the Authors should better define inclusion and exclusion criteria for the study. E.g., did include only patients with final surgical proof of ovarian torsion among those with such a diagnosis? How many 18 years-old and pregnant patients were excluded?

5. Methods, paragraph on "Imaging protocols". A) Why "protocols"? B) Please, detail the number of coil elements. C) Acquisition parameters are too superficially described to guarantee that the Authors results are reproducible. E.g., what about FOV? Matrix? Pixel size? NEX? ETL? Moreover, data are presented confusingly (e.g., page 5, lines 116-18). I believe that a table would be of great help.

6. Methods, page 5, line 24. The TE of the DWI sequences ranges from 62 to 80 msec. However, TE is a crucial factor in determining ADC and image quality, and should as lower as possible. Why such a wide range? Is this a potential bias concerning data robustness/reproducibility?

7. Methods, paragraph on "Image analysis". Please, clarify the reading strategy. Was first reading (conventional MRI only) followed by an interval of time before the second reading (conventional MRI + DWI)? If yes, how long? If not, based on which rationale?

8. Methods, paragraph on "Image analysis". Please, support diagnostic criteria with proper references.

9. Methods, paragraph on "Image analysis", "Quantitative Analysis". Was ADC calculated with linear regression of signal intensity vs. b-values? How large were ROIs (range, mean)? Were ROIs circular vs. manually drawn? Was just one single ROI positioned on a per-patient basis?

10. Methods, paragraph on "Statistical analysis". I wonder whether the chi-square test is the most appropriate one for pairwise comparisons (two different MRI protocols on the same population). Why not the McNemar's test? In this case, its use is inappropriate with predictive values. Please, check with your referring biostatistician.
11. There are too many tables. Please, fuse them into one or two tables, no more.

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.

Unable to assess

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

No

Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?
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I recommend additional statistical review

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