NIMG-23-236: Decision

From: NeuroImage <em@editorialmanager.com>
To: Zhengguo Tan <zhengguo.tan@fau.de>
Reply-To: NeuroImage <support@elsevier.com>
Date: 13 Mar 2023 12:03:25 -0400 (*13.03.2023 17:03:25*)

Ms. No.: NIMG-23-236
Title: Accelerated Diffusion Magnetic Resonance Imaging at 7T: Joint Reconstruction for Multi-Band Multi-Shell Shift-Encoded Echo Planar Imaging (JETS-EPI)
Corresponding Author: Dr. Zhengguo Tan

Dear Dr. Tan,

Thank you for submitting your manuscript to NeuroImage. Your paper has been reviewed by experts in the field. The reviews have been considered by the Handling Editor and the editorial team. Based on the reviews, we regret to inform you that we are unable to accept your manuscript for publication in NeuroImage.

However, while the concerns of the reviewers are serious and make it impossible for us to publish the work at this time, the reviewers did make a number of positive comments. In light of these, if you feel that you could address their concerns fully, we would be willing to consider a revised submission.

Below, please find reviewer comment(s) for your manuscript. If any of the reviewers have uploaded their comments as a separate attachment file, you can view/download the file(s) by clicking the 'View Review Attachments' link once you have accessed this manuscript number. The Editors would like you to consider these comments and revise your manuscript appropriately. Upon receipt, the manuscript will be re-reviewed promptly.

Please note that should you re-submit, your article would be treated as a new submission. Although we generally ask the previous reviewers to consider your revisions, this will not always be possible and it may be sent to one or more new reviewers.

As a new submission your manuscript will be given a new manuscript number with a new date of receipt. In this case, please note in your cover letter that your paper is a resubmission of the previously rejected paper, NIMG-23-236.

Please also supply a detailed list of revisions and responses to the reviews provided with this decision letter, indicating new work undertaken and all revisions made to the manuscript.

***
Data in Brief (optional)

We invite you to convert your supplementary data (or a part of it) into a Data in Brief article. Data in Brief articles are descriptions of the data and associated metadata which are normally buried in supplementary material. They are actively reviewed, curated, formatted, indexed, given a DOI and freely available to all upon publication. Data in Brief should be uploaded with your revised manuscript directly to NeuroImage. If your NeuroImage research article is accepted, your Data in Brief article will automatically be transferred over to our new, fully Open Access journal, Data in Brief, where it will be editorially reviewed and published as a separate data article upon acceptance. The 2015 Open Access fee for a Data in Brief is only $250.

Please just fill in the template found here:
http://www.elsevier.com/inca/publications/misc/dib_data%20article%20template_for%20other%20journals.docx Then, place all Data in Brief files (whichever supplementary files you would like to include as well as your completed Data in Brief template) into a .zip file and upload this as a Data in Brief item alongside your NeuroImage revised manuscript. Note that only this Data in Brief item will be transferred over to Data in Brief, so ensure all of your relevant Data in Brief documents are zipped into a single file. Also, make sure you change references to supplementary material in your NeuroImage manuscript to reference the Data in Brief article where appropriate.

Questions? Please contact the Data in Brief publisher, Paige Shaklee at dib@elsevier.com Example Data in Brief can be found here: http://www.sciencedirect.com/science/journal/23523409
***

The critique of this paper in no way implies a lack of interest in this area of research for publication in NeuroImage, and we hope that you will continue to submit your work to this journal in the future.

Sincerely,

Bruce Pike
Senior Editor
NeuroImage

Reviewers' comments:

Handling Editor: The reviewers find the proposed reconstruction to be original and interesting, however, there are significant concerns about a lack of comparison to current state-of-the-art and a more thorough validation of the method. If these concerns can be addressed, a revision would be considered.

Reviewer #1:
Authors propose an integrated acquisition and reconstruction methodology for accelerated multi-shell diffusion weighted imaging (DWI) based on an interleaved phase-encoding (PE) shifting and joint regularization with local low-rankness (LLR). The motivation is clear. The approach is original, although novelty is only incremental with respect to the state of the art. The methods are generally well presented, although certain details about the reconstruction algorithm are missing. The experimental section is weak, as quantitative comparisons or ablation/sensitivity analyses are missing, although the provided images illustrate the potential of the approach. The discussion is appropriate although some practically relevant points may be missing. The conclusions are generally well supported. Therefore, I would recommend a revision attending the points below.

Major:

1) No quantitative validation is provided. Authors should include quantitative comparisons of alternative methods using ground truth (GT) reconstructions (GT) built either from simulations or from retrospective subsampling of a long-enough DWI scan (or ideally from both).

2) Authors do not compare with JULEP, DAGER or SPA-LLR but these are cited as state of the art methods (more recent than MUSE and MUSSELS). Therefore, authors should also compare with these methods or else precisely indicate why are these left aside.

3) For MUSSELS, authors should explicitly state if they base their implementation on Mani 2017 or Bilgic 2019 (whilst briefly motivating why). For local PCA, authors should more explicitly state whether they are comparing with Manjón 2013 or Veraart 2019.

4) A sensitivity analysis based on the GT should be included considering these factors: LLR regularization weight, LLR block size, LLR overlap factor, PE interleave configuration (i.e., why 2 shots/b?). In case some of these factors are/need to be left aside, authors should precisely justify why. Ablation experiments comparing full proposal versus removing interleaved PE / removing LLR would also be very interesting.

5) LLR regularization performance and reliability may degrade in the presence of motion. Also, often DWI is performed with alternating PEs for distortion correction. SNR is heterogeneous over the FOV, which may not be appropriately covered by a single regularization weight. Please, add these aspects to discussion. See also minor point 5.

6) Abstract is focused on quite general motivating aspects, but paper methods and results are described in one/two sentences each. Authors should rebalance to provide abstract readers a better/quick understanding of paper content. Namely, shift-encoding and LRR ideas should be more clearly described in the abstract.

7) Fig. 2 / Fig. 3 -> you mention slice / diffusion direction included, but this is not very relevant, what matters is to specify the reasons for including these and not others. Also, it would be important to include snapshots at different slice locations within the brain, particularly at inferior locations, where reconstruction may become more challenging. Analogously, please provide rationale for snapshots selected in Fig. 4 and Fig. 5.

8) https://github.com/ZhengguoTan/sigpy seems a link to a generic tool. https://doi.org/10.5281/zenodo.7548595 is not available yet. Please, remember to provide paper-specific links before acceptance or otherwise remove these links from the manuscript.

Minor:

1) Reference to 7T included in title does not seem relevant enough to me. Similar challenges for DWI can manifest for high resolution / high b-value lower field scans as well, so I'd recommend removing reference to 7T in title.

2) L55: "DAGER requires many diffusion directions" -> JETS also seems to require many diffusion directions, so guess this should be formulated differently?

3) Section 2.2.1 and 2.2.2 -> please, explicitly specify slice thickness.

4) L105: "acceleration" -> would use "undersampling" as this is EPI and there's no 1-1 correspondence between undersampling and acceleration (several occurrences).

5) L160: "As phase images are spatially smooth" -> this may be arguable, would suggest replacing by "Assuming that phase images are spatially smooth". Think you should add a bit more on this topic to discussion, as phase behaviour depends on several hard-to-control factors such as pulsatile motion, its impact at different locations within the brain, diffusion sensitization strength, bulk motion,... so phase correction may sometimes be really challenging? Some additional lines of discussion on integration of navigators / cardiac gating could also be worthwhile.

6) L172: "firstly slides [...] matrices" -> not very clear, think you should reword and explicitly mention what dimensions are concatenated in rows and columns of matrices. Also, reasons for not using 3D patches should be discussed.

7) L176: "T^HT input neq input" -> unclear, maybe a typo?

8) L181: "efficient implementation" -> claim on efficiency does not seem supported from description... inverse density weighting is well-known for reconstructing original data levels back

when slide-windowing. You should provide more details on efficiency or articulate description differently. Importantly, overlap ratio does not seem to be reported, but this may have a dramatic effect in computational cost.

9) Supporting Figure S3. "Small block size (i.e., 2) suffers from image blurring, whereas increasing block size gradually leads to increased noise" -> may appear counter-intuitive as small block sizes should aid with localization and therefore prevent blurring, at the price of less information for denoising? Can you clarify on reasons / potential hidden factors for this behaviour?

10) L239: "blurring" -> leading artifact resembles Rician bias rather than blurring in my opinion, could you clarify?

11) Seems Fig. 4 could be replaced by encompassing Supp. Fig. 6?

Typos, suggestions...

- L33: "needed" -> "used".
- L83: "benefits" -> "benefits" to be stated experimentally, in methods better to refer to "properties"?
- L112: "as Section 2.2.1" -> "as in Section 2.2.1".
- L119: "DW acquisition" -> "DW acquisition volumes".
- L120: "demonstrates" -> "will be used to demonstrate".
- L123: "slice collapsed k-space data" -> not sure this expression is clear, you may consider rewording.
- L125: "Such acquisition can be modeled in two ways." -> "Acquisition modeling needs to consider several aspects.".
- L134: "at every" -> "of each".
- L136: "shot images per" -> "multiple shots acquired for a given".
- L137: "One method" -> "A possibility".
- L141: "is done" -> "can be done".
- L142: "This method can be written" -> "This can be incorporated to our formulation".
- L147: "(e.g. Hanning window)" -> would remove as there are many other possibilities, so this reference may not contribute to clarity of description?
- L148: "phase correction method" -> "phase correction".
- L152: ", utilizing the concept of object-oriented linear operator abstraction" -> not sure this is adding much, would remove.
- L168: "Intuitively, low rankness comes from contrast variation feature of DW images" -> meaning unclear to me, you may reword.
- L173: "patchs" -> "patches".
- L176: "as an" -> "as a".
- L185: "The acquired raw data was read by twixtools (URL)" -> not adding much, could be specified in code repo.
- L191: "the proximal operator" -> "proximal operator".
- L196: "x E" -> better to separate rather than concatenate description for both.
- L221: "GFA" -> acronym does not seem defined.
- L247: "desnoing" -> "denoising".
- L252: "within the rectangular regions in Fig. 4" -> these may be difficult to appreciate, consider enhancing a bit perhaps?
- L259: "smooth patterns" -> "smoothness".
- L261: "spatial-angular" -> really angular or b-vals are fused? Perhaps more accurate to say "spatial-diffusion"?
- L271: "achieves" -> "uses".

Reviewer #2: See attached file

Reviewer #3: # Summary

The authors present a novel approach to accelerate diffusion imaging acquisitions by jointly reconstructing highly accelerated diffusion-weighted images recorded with different diffusion weightings and k-space sampling patterns. The underlying assumption is that by encoding complementary k-spaces in the different diffusion images, they can acquire a smaller k-space for each individual image, thus accelerating the overall acquisition. They show that their algorithm is able to reconstruct this undersampled data, whereas other contemporary reconstruction algorithms (which do not jointly reconstruct the data) perform less well.

The principle is interesting, but I feel that the Authors are missing some important validation aspects, which are detailed as Major comments below.

Major comments

1. How many subjects were actually scanned? The Materials and Methods refers to "healthy volunteers", but I could not find an explicit number. The data presented in the paper seems to come from only a single subject. This is obviously inadequate to properly evaluate the performance of the method.

2. Was there any correction for different eddy currents and motion between volumes? If not, why not? Eddy currents and (out-of-plane) motion could break the assumption that the low-rank patches reflect the same underlying anatomy. Perhaps the joint reconstruction method developed by the Authors actually mitigates these effects (and that would represent an additional advantage of the method), but correction would definitely need to be done in the case of the other reconstruction methods in order to make a fair comparison.

As an aside, the Authors do refer to "motion robustness" several times, but this can surely only be robustness to in-plane motion. Out-of-plane motion in 2D imaging is harder as it is accompanied by a true loss of information. Model-based methods like that implemented in eddy (Andersson, et al. Neuroimage (2017) https://doi.org/10.1016/j.neuroimage.2017.02.085) can partially compensate for it, but don't seem to have been used here; and techniques like gSlider can correct for motion within the thick acquired slab during reconstruction (Wang, et al. Magn. Reson. Med. (2018) https://doi.org/10.1002/mrm.27196), but require specialised acquisitions.

I would suggest that the Authors properly discuss the issues of eddy currents and intra- and inter-volume motion, and perhaps consider how they could properly incorporate a consideration of these effects into their framework.

3. Similarly to 2.: was there any correction for susceptibility distortions? I understand that the segmented EPI will have less distortions than conventional single shot EPI, but they still need to be corrected for to get anatomically correct images. Was the GRE scan perhaps used to make a B0 map which was included in the reconstruction?

4. It did not become clear to me why this study is being done at 7T. The increased sensitivity to B0 distortions and SNR loss with long TEs which this study proposes to overcome are 7T problems that are much less pronounced at 3T. In part it is for this reason that most DWI studies are still performed at 3T. What benefit are the Authors aiming to get from running DWI studies at 7T?

5. Relatedly, the acquisition scheme doesn't seem that fast. A similar scheme at 3T could even be shorter as T2 signal loss is less pronounced and so each (multiband) slice could be acquired in a single shot.

6. The denoising approach used as comparison does not reflect the state of the art. It is generally recommended to perform denoising on complex data (see Cordero-Grande, et al. Neuroimage (2019) https://doi.org/10.1016/j.neuroimage.2019.06.039), as this will avoid the noise floor issues

that are apparent in the MUSE + Denoiser panel in Fig. 3. This should be completely possible for the Authors, as they reconstruct the data themselves. I would say the only reason not to do complex denoising at this point is if you are stuck with only scanner reconstructed magnitude data. Complex denoising is available in openly available toolboxes, e.g. MRtrix3 (https://mrtrix.readthedocs.io/en/latest/reference/commands/dwidenoise.html).

7. While we are on the topic of "state of the art": All the papers in the Introduction demonstrating "conventional" SS-EPI are from over 20 years ago. There have been important technological developments, e.g. Connectom scanners, gradient inserts, sequence developments, e.g. multiband, gSlider, and image correction technique developments, e.g. the continued development of "topup" and "eddy", since then.

8. The Introduction is in general very confusingly written, and I would suggest considering how to put the introduced topics in a more logical order. As I see it, the fundamental problem is the trade-off between minimising distortions and maximising SNR while minimising acquisition time and sensitivity to motion, and this message does not come through clearly. For instance it is very confusing that it is suggested that navigators should be avoided because they increase the acquisition time, but segmented EPI -- which literally (at least) doubles the acquisition time while also introducing motion sensitivity -- is introduced as necessary without a consideration of the trade-offs.

9. How were the number of directions in the three shell case determined? Generally 30 directions are recommended even for b = 1000 s/mm2, but here only 20 are used.

10. The "efficient implementation" to correct checkerboard artefacts (lines 177--183) seems very underspecified. Perhaps it would be better developed in an appendix and just mentioned in the main text? Specific points:
- is (1/divisor) a matrix inverse of (T'*T*1)? Or is it rather a scalar derived by solving the linear equation (T'*T*1) = (something)?
- is 1 the matrix of all ones, or the identity matrix?
- why would including this divisor prevent (or mitigate) checkerboard artefacts at all?

Minor comments and typos

- title should probably say "Diffusion *Weighted* Magnetic Resonance Imaging"
- typo in graphical abstract (METHODS (2): "reconsturction" instead of "reconstruction")

Abstract

- high b-values do not "increase ... noise"; this should probably be rephrased to make clear that more strongly diffusion weighted images have lower SNR, thus increasing the *sensitivity to* noise.
- "inplane" should be "in-plane" for consistency with the rest of the document.

Introduction

line 22: "spiral" is not a multi-shot EPI technique.

line 37: should be "single shot images", not just "shot images"?

line 57: should be "in-plane".

line 58: should be "still require long acquisition times".

line 68: "i.e." should be "specifically".

line 69: "the established DW image denoising algorithm, i.e., local PCA [REFS]" would be better written as "an established local PCA-based DW image denoising algorithm [REFS]" (though see the major comment above regarding whether this is state of the art).

Materials and methods

line 73: should be "Materials and methods" (not "Material").

line 92: the non-standard way of representing maximum gradient strength and slew rate should be written more explicitly.

line 107: why are the b=0 images referred to as b = 50 s/mm2 images here, but b_0 acquisitions for the three shell protocol? This should be standardised.

line 107, 119: why is minutes'seconds'' notation used here but "min" elsewhere? Recommend standardising, especially since the used of minutes'seconds'' is fairly old-fashioned...

were the different b-values in the second acquisition scheme interleaved, or collected one after each other?

line 119: should be "acquisitions".

line 139: presumably this should be "This method is robust to in-plane motion".

line 176: should be "a proximal operator".

line 177: "Noteworthy" is not usually used in this sense. Here it seems redundant and can be deleted.

line 180: I suggest "Hermitian adjoint" rather than just "adjoint".

line 183: should be "the input".

line 194: the meaning of the \mathcal{T} symbol should be defined.

line 196: again, "Noteworthy" is an odd choice here. I would suggest "Importantly".

line 196: missing "and" between "x E".

line 199: should be "conjugate gradients".

line 203: should be "as" not "ss".

line 206: NVIDIA seems to write the name of the GPU as "NVLink".

line 209: again, "i.e." should be "specifically".

line 212: "implemententation" should be "implementation" and quotes are backwards (should be ``).

lines 217–218: which shell or shells were used to compute the fODFs?

line 221: "GFA" abbreviation should be defined.

Results

line 228: should be "loses".

line 241: "allows to resolve" isn't a standard English construction. Should be something like "allows the resolution of".

line 247: should be "denoising".

Discussion

line 265: "dubbed as" is redundant; can just be "dubbed".

line 273: should be "diffusion-directions".

line 281: instead of "as" I would suggest "as used by".

line 289: instead of "on GPU A100" I would suggest "on an A100 GPU".

line 305: should be "solves for a fewer number of".

#AU_YNIMG#

To ensure this email reaches the intended recipient, please do not delete the above code

---

In compliance with data protection regulations, you may request that we remove your personal registration details at any time. (Remove my information/details). Please contact the publication office if you have any questions.