Reviewer’s report

Title: Estimating view parameters from random projections for Tomography using spherical MDS

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Reviewer: Deshan Yang

Reviewer’s report:

The paper proposes the sMDS method to sort the projection data that are measured at unknown view angles. The method involves 1) computing the pairwise distance matrix $M$ on Fourier transformed projection data, 2) finding the first two eigenvectors of $\text{Tao}(M)$, 3) Computing the initial angles using the two eigenvectors, 4) uniformly rearranging the view angles along the circle. Image reconstruction from projection data after viewing angles are recovered is less concerned in this study. A few experiments are conducted to study the effects by the number of projection angles and by levels of detector noises. A 2D MR brain image is used in the study, with all projection data and noise levels are simulated.

Major concerns:

1) Are there particular reasons to compute distances between projections after Fourier transformation, instead of distance on the original projection data themselves? I cannot find any discussion about the benefits, and comparing to results using the original projection data without Fourier transformation. In fact, if it is working fine to compute the distance on the original projection data, the whole thing about Fourier transformation, and central slice theorem can be removed, and the method can be greatly simplified.

2) The whole discussion about dimensional reduction (Figure 7, from $F$ to $Q$) is not very useful. The only useful information about the vector $Q$s are their direction, or the angles, that are equal to (in one way or another) the viewing angles of the original projection data. The thing about dimensional reduction is basically about “ok, each projection is done on an angle, and angles are the dimensional reduction of the projection data”. Am I right?

If I am correct, the proposed procedure could be simplified, and related discussion can be removed.

3) I suggest authors to remove anything referring to 4DCT and motion artifacts reduction. The proposed method won’t be very useful on such problems in that the tissue motions are mostly in/out the 2D slice instead of within a 2D slice. The 3D implementation of the proposed method won’t work either for 4DCT problems since CT projections can only be in axial directions.

4) Without discussion about how tissue motion affects the projection, I fail to see the usefulness of the proposed method (and other previously published similar methods) in real-world applications, e.g. MRI scanning with patient motion.
Problems with such applications are really not about unknown viewing angles, or at least the problems cannot be solved by uniformly rearranging the recovered viewing angles. BTW, MRI imaging is not, like CT imaging, by line integral projections.

This may bring up the question that the example applications discussion in the paper maybe not the good candidates for this proposed method, even though other published papers also refer to the same applications.

5) Authors may consider to somehow rearrange the flow of this paper, to make the flow more linear. There are multiple examples that one thing is briefly introduced, and then explained in details in a far later section. For another example, is Figure 4 really useful since it is pretty much the same as Figure 1B and C?

6) Writing is good in general, but need to tighten up.

7) Since the most important contributions of the manuscript is a new method to recovery projection angles (or order or angles), shall authors consider to present some results of the recovered angles versus the known ground truth (known angles before projections are randomized), and cases when the method fails? Such results maybe useful to directly/indirectly compare to other published methods.

8) I certainly like to see some results on real data, instead of simulated data. (But I doubt this will happen)

9) I don’t feel Figure 5, which analyzes the method on different number of projection angles, is important, unless the authors want to show this method is superior than other published methods for these situations.

Other comments:
1. Shall the results section in the abstract be divided into two sections “method” and “results”? It is usually not enough to tell only the results in the abstract without mentioning the methods.

2. In fact, I cannot tell how view parameters are estimated from the abstract, which means the abstract is not adequate.

3. I see some confusion among the terms of “object orientations”, “view angle”, and “acquisition positions”. In the 3 examples you give in page 2, I agree that the “object orientations” are changing during the measurements (collection of the line-integral projections), but the “view angles” and “acquisition positions” are apparently well defined (respecting to the detectors, or the CT scanners) without uncertainties. They are certainly uncertain respecting to the objects, but the object orientations are not. Besides, you may really mean the internal objects in the patient body, but not the whole patient as an object. Please try to make it clearer in the introduction section (or/and in the abstract)

Minor things:
1. References please at section 1.3, line 3
2. Please add line numbers for the entire manuscript
3. In Fig 7, the vector Q1 does not seem to be perpendicular to F1. Q2 is ok, though.

**Level of interest:** An article of limited interest

**Quality of written English:** Needs some language corrections before being published

**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