

## ***Interactive comment on “Reconstruction of 3-D cloud geometry using a scanning cloud radar” by F. Ewald et al.***

### **Anonymous Referee #2**

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It's an unusual paper. Based on its title I expected to read how cloud 3-D structure (liquid water path, droplet size and optical depth) can be retrieved from scanning radar measurements. Instead it is more on radar scan resolutions and interpolation between them. After reading this manuscript to the very end, I've realized that it complements to Fielding et al. (2013) paper on using ground-based radar to retrieve 3-D cloud structure rather than overlaps with it. Perhaps, the title of the manuscript should be clarified in order to find its specific readership. The paper definitely deserves to be published in the AMT but after substantial changes. Some of them are suggested below.

1. The introduction is too short and a bit misleading; it briefly talks about the problems in remote sensing of inhomogeneous clouds but does not link them with the goals of the current paper. From the other hand, some of the statements in the introduction are

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hard to interpret. Specifically, - what is “the part of the cloud oriented towards sun and sensor” and why the Nakajima-King technique works only there (lines 9-10); - what are “the complex-shaped cloud edges” and how they are compared to the “simple-shaped cloud edges” (line 19); - what is meant by “the unknown cloud surface orientation” in line 21; - what is “volume reconstruction” in line 25. Finally, I believe that the introductory section needs a better description of Fielding et al. (2013) paper and its link to the current manuscript.

2. Unfortunately, I am not familiar with the different interpolation methods used in the manuscript: nearest-neighbour interpolation, Shepard method, barycentric interpolation, and natural neighbour interpolation (section 3.3). What is the difference between them? How does the “natural neighbour interpolation” differ from the “nearest-neighbour interpolation?” It was hard for me to appreciate the comparison done by the authors and their conclusion to choose the “barycentric interpolation.” This is especially true since the RMSE for all four interpolations are very close as shown in Table 3.

3. Why the “resolutions coarser than approximately 2 to 4 have to be avoided.” According to Table 2, the difference between 4 and 5 degrees resolution is negligibly small. The difference is also hard to see clearly in Fig. 3. Perhaps, the choices of resolution should be related to specific applications.

4. The analysis of power spectrum density is very interesting. However, it is difficult to interpret it without understanding of what each interpolation does. Also, why the “true” spectrum is larger even for large scales?

5. I would recommend adding a droplet size distribution to the field in Fig. 1.

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Interactive comment on Atmos. Meas. Tech. Discuss., 7, 11345, 2014.

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