Atmos. Meas. Tech. Discuss., 7, C4255–C4258, 2014
www.atmos-meas-tech-discuss.net/7/C4255/2014/

Atmospheric Measurement Techniques Discussions

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

Anonymous Referee #1

Received and published: 18 December 2014

This manuscript investigates 3D reconstruction in cloud geometry for passive remote sensing applications, using synthetic and actual measurements of scanning cloud radar. The manuscript presents important work and is well explained and relatively easy to read. However, I would hope that the authors could address the following issues to make this manuscript fully present what the authors set out for.

General comments:

It is too bad that the authors end this manuscript with two cloud images without any quantitative error measures between radiance estimated from the reconstructed cloud field and radiance observed from either camera or imager. For a scientific paper, the authors may wish to improve this bit a little more. I understand it may be difficult

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

because it involves instrumental calibration and assumptions used in Monte Carlo simulations, etc. However, visualised similarity in photos cannot be directly translated into a success in cloud retrievals from shortwave radiance measurements. As the authors point out, the smallest RMSE in 870-nm radiance doesn't necessarily correspond to the best performance in liquid water content field. Similarly, I am not sure that the smallest RMSE in radiance will correspond to the best performance in cloud retrievals. If the authors cannot address this issue in real observations, at least, this should be relatively straightforward using synthetic measurements.

Following the comment above, it would make more sense if reconstruction performance is also evaluated, for example, at 1640 nm to see the impact of cloud droplet size, which is an important factor for scanning cloud radar. I understand that the authors are trying to keep the droplet size constant so the scan strategy and interpolation methods can be isolated and properly evaluated. But again, if the authors emphasises the primary impact of their work is on passive remote sensing including cloud side retrievals (by the way, the references Marshak et al. (2006) and Zinner et al. (2008) both focused on cloud droplet size retrievals!), I think addressing this issue would greatly enhance the value of the manuscript.

It is a bit weird that the manuscript talks about Figure 1, then jumps to Figure 4, and then comes back to Figure 2, etc. Could the authors please sort out the order of the figures properly?

Specific comments:

1) Page 11347, Line 19–21: I am a bit confused about this statement. The authors seem to suggest that knowing complex-shaped cloud side at spatial resolution for 100m or less is important, but the reconstructed cloud field shown in the manuscript has a resolution at 100 m at best. Shouldn't this imply that even scanning cloud radar measurements wouldn't be good enough for cloud side retrievals? Could the authors please clarify?

AMTD
7, C4255–C4258, 2014

2) Page 11351, Figure 1 and Figure 3&4: Figure 1 shows column-integrated properties of clouds in a (x, y) coordinate, while Figure 3&4 represent quantities in a (θ, Φ) coordinate. I found Figure 1 not very helpful because one needs to imagine how radar scans in order to match Figure 1 and Figure 3&4. The authors may wish to provide a 3D plot or some cross-section profiles to help readers understand the text. Additionally, perhaps I miss it, but I don't think it is clearly mentioned where the radar is located until Figure 8. I could guess it is in the bottom-left corner from the text, but this kind of information should be clearly put.

3) Page 11353, scan strategy: The authors directly point out that S-RHI is better (or favoured) than S-PPI, due to the consideration of cloud advection. Could the authors please confirm whether thorough evaluation has been done to draw such a conclusion? Results may depend on wind speed and whether wind information is available, etc., so the authors may wish to consider providing conclusion/discussions from all aspects. Words like "never", "impossible" are strong words that should be avoided; we are all doing something that has been thought "never" in the past.

4) Page 11354, Line 9: small typo "then" should be "than".

5) Page 11355, Interpolation methods: Since the section of interpolation methods immediately follows "Remapping radar data to Cartesian space considering cloud motion", readers may wonder if the authors perform interpolation in reflectivity or liquid water content. I believe the answer should be liquid water content, but it would be better to describe this kind of important detail clearly.

6) Figure 5: I cannot understand what has been shown because x- and y- label and values are all missing. Neither text nor figure caption mention where this cross-section has been chosen with respect to Figure 1, 3 and 4, and why. Additionally, how do those radar scans in Figure 5b correspond to text in 3.2 and 3.3? Are they the fields after remapping? If they are, shouldn't they be in irregular grids? Overall, the figure and descriptions need to be greatly improved.

AMTD

7, C4255–C4258, 2014

7) Figure 6: Information on scan resolution should be included in caption.

8) Figure 7: Could the authors please indicate which line (or boundary) represents 1° resolution and which one represents 5° resolution? I know one can compare Figure 7 with Figure 6 to identify which one represents 1° resolution. The reason I ask is because these two lines seem to switch their order in the higher frequency regions for Barycentric method. Could the authors please confirm that? If indeed two lines switch the order, what does it mean physically and why?

Hope it helps.

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