

Interactive comment on “Improved rain-rate and drop-size retrievals from airborne and spaceborne Doppler radar” by Shannon L. Mason et al.

A. Protat (Referee)

A.Protat@bom.gov.au

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Review of “Improved rain-rate and drop-size retrievals from airborne and spaceborne Doppler radar”, by S. Mason et al.

This paper presents a new variational retrieval technique to estimate rain rate and information about the drop size distribution from airborne 94 GHz Doppler cloud radar data. This work is important given the perspective of the launch of the first 94 GHz Doppler cloud radar in space. I recommend this paper be accepted provided that the comments below are addressed prior to publication. My only “major” comment

General comments :

1. Page 11, line 5: Since you don't have independent measurements to validate your

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algorithm, you have elected to use Z9.6 and BV9.6 as an indirect validation, which is OK but is certainly a limitation in the paper. It would be great to make the most out of it. So I wonder why you have decided not to show the whole vertical distribution (and maybe difference plots). That would characterize the errors in a more exhaustive way. A major improvement to the paper would be to demonstrate that with the limited validation you have, you can show that the vertical distribution is captured.

2. Discussion on case study 2, Fig. 8: No comment on the vertical distribution of D0 for that difficult case? In evaporation conditions is it realistic to see D0 decreasing at lower altitudes? The fact that Nw is held constant should be an issue here, as one might expect Nw to strongly diminish lower in height, and maybe D0 increase due to removal of the smaller drops evaporating? That is one illustration of my general comment 1. You need to show the vertical distribution, not a selected height, because it does not tell the full story.

3. Page 17, lines 23-24: You say that in this study you have used measurements to investigate the prospects for improved global rain rate retrievals from spaceborne Doppler radar. However in order to study that more completely you could have chosen to simulate from these aircraft observations what EarthCARE would actually measure, and whether you would be able to get similar results between degraded spaceborne measurements and higher-resolution higher-quality airborne measurements. I feel this piece of work is missing to really make that claim. In that respect I would remove mention of “spaceborne” in the title of the paper, not to give the impression that you are actually improving satellite rain rates with this technique, as I don't think you showed that.

Specific comments :

1. Page 1, line 11: separate “between” and “light”.
2. Page 3, line 10, “. . . unattenuated wavelenght . . .”. This is incorrect, there is still large attenuation at X-band. This needs to be rephrased and errors associated with

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potential X-band attenuation assessed.

3. Page 3, line 15, “dual-radar” : do you mean dual-frequency radar ?

4. Section 2.3.4 title : What about 94 GHz attenuation due to graupel or hail ? Something needs to be said about that in this paper.

5. Page 8, line 5 : “assume multiple scattering effects are negligible ...”. You should probably explain why you think that is reasonable (very small beamwidth). A comment is also needed to explain that this would need to be done in a spaceborne application.

6. Page 8, line 24: “as 3 dBZ, and as 0.5ms⁻¹ for mean Doppler...”. This seems large for reflectivity and too small for Doppler velocity. Vertical air motions can be 1-2 ms⁻¹ in the lower troposphere easily in the clouds you are interested in ... That brings up a question you need to address (sorry ...): how sensitive to this value are the results ?

7. Page 8, line 27, “ .. by liquid water ...”. What about melting ice, graupel, hail ?

8. Page 14, line 31-32: What about at lower height ? I would expect that if the Nw assumption is not satisfied lower down in the evaporating area, then D0 should have more errors and then Z(9.6 GHz) would be less good. It would actually be more interesting to show the whole vertical distribution instead of extracting one height for all these plots to demonstrate if the Nw assumption does create discrepancies on the vertical distribution of Z9.6. Hence my general comments 1 and 2.

9. Section 4.3.1, title: I think you could go to 12:46 for your comparisons? Why did you stop at 12:45 ?

Good luck with the review.

Alain Protat, Melbourne, May 2017.

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