Atmospheric Measurement Techniques - Manuscript AMT-2023-1118: "Quantifying riming from airborne data during HALO-(AC)^3" by N. Maherndl, M. Moser, J. Lucke, M. Mech, N. Risse, I. Schirmacher and M. Maahn

1 Summary

This manuscript presents two new methods to quantify riming on ice particles using the normalized rime mass M from airborne in situ and radar measurements. The first method combines in situ and remotely sensed radar observations in the configuration such that the radar carrying aircraft is overflying the in-situ probe carrying aircraft, while the second approach is based on in situ observations only (less demanding in terms of aircraft flight pattern). The two methods are shown to produce similar estimates of the normalized rime mass over the data collected during the HALO-(AC)^3 campaign that took place in the Arctic near Svalbard in Spring 2022, in a statistical sense. Two case study are further investigated. The first one (based on combined radar and in situ data) suggests that the the regions characterized by a higher normalized rime mass are related to regions exhibiting higher reflectivity values. The second one (based on in situ data only) illustrates that riming may occur in regions with low liquid water path and hence suggests that riming may occur in layers above, containing more liquid water.

2 Recommendation

The manuscript is clear, the methods are properly described, as well as the associated assumptions and limitations. The topic is of interest to the community and readership of AMT. There are however a few questions/issues to be clarified (see list below), and I recommend to send the manuscript back to the authors for major revisions.

3 General comments

1. My main concern about the evaluation of the two methods is related to Fig.5 and also Fig.7 to some extent. My take from the scatter diagram in Fig.5 and the two curves corresponding to the two methods in Fig.7 is that the two methods agree on the overall shape of the distribution of the M values, in a statistical sense, but are not cofluctuating (also confirmed in case study 1, see Fig.10). In addition, most of the M values are low and the 2 methods do not seem to agree well on the very few high values, leading to a density of points that is not at all aligned with the 1-1 line in Fig.5. In Fig.7, the two methods apparently agree well for the low M value (in log scale...) but again not that much for the large M values. So I am wondering if the two methods are really in good agreement or if the data set is too unbalanced to provide a robust answer. I think this issue should be

clearly discussed in the paper, and the limitations of the comparison performed should be better emphasized.

2. In section 3.1, it is mentioned (l.217-218) that the prefactor and the exponent of the mass-size relationship are taken for dendrites. What is the influence of this choice on the retrieval of M in clouds with other habits than dendrites? I did not find the discussion on this assumption.

3. In section 4.1, the vertical profiles of M are discussed and linked to environmental conditions (temperature, LWC, LWP...) but I am wondering what the uncertainties associated with the retrieved M values (and subsequently on the rimed fraction) are. And in particular if the shapes of the curves are statistically significant. As Sa is taken about 1, my gut feeling (and I may well be wrong) is that the uncertainty associated with small M values (the vast majority of the cases) is relatively large and may induce limited significance. Such uncertainties are displayed in Fig.10 for instance, why not in Fig.7? This would strengthen the analysis of the shapes of those curves (or suggest that those are not statistically significant).

4. I am mot sure I understand what it is added value of the case study 2: rimed particles are detected in regions with rather low LWC, therefore there must be layers with higher LWC above, or more generally there is not enough information about the context above the aircraft to draw any solid conclusion. So nothing original here, and I do not think it is worth being mentioned in the conclusions (see l.493-495). If this is the case, I suggest to remove the 2nd case study.

4 Specific comments

1. P.5, l.108: what is the influence of the choice of those parameters for the time and space consistency on the optima estimation parameters (e.g. covariances)?

2. P.7, l.171-175: I did not understand how the LWC values were estimated along the radar beam (in order to quantify the attenuation), this should be better explained.

3. P.7, l.186: I think it should be "for" instead of "to" before "our results".

4. P.9, l.235: is Z_e expressed in dBz or mm^{6}m^{-3}?

5. P.9, l.237: "to make S_a more Gaussian": maybe showing a distribution (in appendix?) would strengthen the claim?

6. P.9, l.239: given that S_y and S_a are of the same magnitude, does the insensitivity to S_a imply that the 1st term in Eq(3) is dominant and hence that F() is strongly conditioning the retrieved values?

7. P.11 l.286: it should be Fig4.b, no?

8. P.13, l.307: it seems that "a" in between "point" and "perfect" should be removed.

9. P.13, l.312: the RMSE value seems much larger than the mean value, which suggests strong uncertainty no?

10. P.14, l.14: "simIlar"

11. P. 16, l.360: ' 'Figure 8 analyses the dependence of temperature and LWC on M": should it be the other way around?

12. P.18, l.411: I suggest to add "in terms of temporal cofluctuations" after "agreement" to clearly emphasize on what this agreement is.

13. P.20, Fig.10: the dashed line in plots (b), (d)... is not explained in the caption.

14. P.23, l.498: "depended": should it be "depending on the"?

15. P.23, l.513: the units of N and N_0 should be $\mathrm{mm}^{-3}\mathrm{mm}^{-1}$, as $N(D)dD$ is the concentration of drops of size between D and $D + dD$. This is consistent with the definition of Λ 3 lines after (in its current version, Λ would be dimensionless).