Atmospheric Measurement Techniques Discussions

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Reply to referee comment 2

Alan Geer

Author comment on "Physical characteristics of frozen hydrometeors inferred with parameter estimation" by Alan J. Geer, Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2021-50-AC2, 2021

Thank you for your review and for your comments. The main issue of supercooled liquid water (SLW) in deep convection is an important point and I appreciate your introduction to the literature on this. As suggested, I will add some discussion, likely in the conclusion of the revised manuscript. The basic point is well taken that the existence of lightning in deep convection, and the rimed particles that generate it, requires significant mass of SLW above the freezing level, likely both raindrops and cloud liquid (e.g. Kumjian et al., 2012; Fuchs et al., 2018). The IFS model broadly does not represent SLW within convection. This issue has already come up, for example in the need to detrain a higher proportion of SLW into stratiform cloud in order to correctly represent shallow convection in cold air outbreaks (Forbes et al., 2016). It would be possible to use the parameter estimation framework to explore alternative partitioning between ice and liquid, as you propose in minor comment 4, however I would like to leave such a substantial additional piece of work to a future study.

My guess is that properly representing SLW will make the representation of strong scattering brightness temperature depressions in deep convection even harder. As illustrated in Geer et al. (2021), above 100 GHz, the bulk optical properties of rain provide a single scattering albedo (SSA) of around 0.5, as compared to around 0.95 from completely frozen particles. This means that liquid droplets above the freezing level will likely provide strong absorption and emission, so that they will be able to increase the brightness temperature of the cloud towards the physical temperature (e.g. 200K) and away from the scattering-dominated brightness temperature of the cloud (e.g. 100K or less). These mechanisms are described in a little more detail in Geer et al. (2021). I will try to make this point compactly in the conclusion of the revised paper.

On the minor points:

1) Noise in observed radiances is not considered at any point in this work. Currently, noise is irrelevant to the modelling of cloud and precipitation-affected radiances, where the modelling errors may be at least 10 K in brightness temperature, and the instrument noise may be around 0.5 K (e.g. MHS, https://space.oscar.wmo.int/instruments/view/mhs). This point is explored in more detail in Geer and Bauer (2010); I will see if it is possible to mention this in the description of the cost function in section 4.2 of the revised manuscript; certainly there is no need to represent the true observation error explicitly in

the parameter estimation.

2) On the mixing ratio adjustment, yes this is done simultaneously at all altitudes. This point will be clarified in the revised manuscript.

3) On the validity of the assumption of linearly additive perturbations, yes, the larger errors of 20 K are predominantly in convective areas over land (as judged by plotting them on a map, for example). These are the locations where the revision to the cloud overlap has had most effect on the brightness temperatures, seen also in Fig. 8 in the submitted manuscript. Another way to quantify these errors is as a fractional error: by this measure, the assumption of linear additive perturbations underestimates the larger TB (>10 K) increases by typically around 30%. Also, on revisiting these results, I discovered one mistake in the text: the overall standard deviation of the differences is 0.96 K, not 0.2 K as stated. These numbers and additional discussion will be added to the revised manuscript. Another way to judge the validity of the assumption is to compare maps of the mean and skewness measures that are used in the cost function; these are nearly identical to a visual inspection (based on figures equivalent to Figure 7 in the submitted manuscript). The assumption is not perfect, but it was vital as a way of making this work practically achievable. Based on this discussion, I would also like to add a line or two to the conclusion to say that future work should include further testing of the validity of this assumption; it should not be just relied upon by future investigators.

4) Yes, as mentioned above, it would be possible to explore alternative partitioning of the ice and liquid in convection. Some discussion around the importance of supercooled liquid water in convection will be added, probably in the conclusion of the revised manuscript.

5) This is a good suggestion to be more precise about the model resolution and the satellite field of view in the first sentence of the abstract; I will include it in the abstract of my revised submission (word limit permitting).

References

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