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Comment on amt-2021-50

Anonymous Referee #3

Referee 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-RC3, 2021

This article describes a major upgrade and its physical justification of the frozen hydrometeor parameters in the radiative transfer model of RTTOV relevant to microwave and submillimeter wave spectrum. In the macro-scale, this upgrade revised a cloud overlap scheme and introduced a new "convective snow" that is analogous to the cumulus parameterization in non-cloud-resoling models. In the micro-scale, new particle size distribution and ice/snow particle shapes are introduced. In the parameter selection procedure, the cost function is configured to match the PDF between calculated and observed brightness temperatures, which is innovative. I found that the author has done a thorough examination to this parameter selection problem and worked his best in this inter-tangled web of uncertain parameters to find the best configuration for practical use. The effort is commendable. The structure of the paper is logical, and the writing is easy to follow (although a little lengthy). I recommend accept after some minor revisions. The following are my concerns.

- Although the new (Final) configuration seems to be more physically sound, as a reader, I am not clear how the cost decrease from 0.932 to 0.911 translates to real improvement. I believe that Figure 5 is intended to show the impact, but neither the figure nor the explanation make me feel clearer about the impact. Therefore, I'd like to suggest: (1) improve Figure 5, so that it can show the result from the "Best" is closer to "Observations" than "Control", (2) add more explanation to argue the "Best" is better than "Control", and (3) add a PDF figure to show the PDF shifted more closer to "Observations" when "Control" is replaced by "Best". The author may also try some other ideas. The point is: how to translate the cost decrease to easy-to-understand improvements.
- The "snow mixing ratio" dimension is peculiar. As I understood it, it is rooted from the uncertainty in particles' falling speed when you convert from mass flux to water content. But still, altering mixing ratio doesn't seem to be a sound approach. Fortunately, in the "Final" configuration it is decided not to change it. I'd like to propose to eliminate this dimension in the first place.
- The situation-dependent results listed in Table 6 are interesting and make us think more about the physics in terms of how to select ice particles. Clearly, the "snow"

particles in the tropics and higher latitudes are different – all the results seem to point out that tropical snow particles are denser than those in mid-latitudes. Or, maybe the "convective" and "large-scale" separation of clouds is not an ideal one after all. Microphysically, the anvil associated with tropical convections are distinctively different from the stratiform clouds associated with frontal large-scale uplifting. This may be an area of future improvement of the "best configuration". Adding some statements along this line in the conclusion section will be helpful.