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Atmos. Meas. Tech. Discuss., author comment AC3
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Reply to referee comment 3

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-AC3, 2021

Thank you for your review and for your comments. The point about the length of the paper, consistent with reviewer 1, is taken onboard for future work, but it would be hard to adjust the manuscript now without cutting important details. Responding to the three concerns:

1) The point is taken that it is hard to get a sense of the improvement going from the control to the improved configurations, based on the global measures of cost. However, Figure 5 is intended to show the shape of the cost function and to illustrate the technique, whereas Figures 7 and 8 are already provided to try to illustrate the changes, such as improvements in simulating extratropical convection over land, and slightly worse fits in tropical convection over land. I will try to add the PDF of simulated and observed brightness temperatures to the revised manuscript, as suggested. However, although it shows improvement, this is not very large. The problem is that there is not a big overall improvement in fits to observations (and I hope the manuscript does not imply this). The reduction in cost from control to the "best" configuration is only from 0.925 to 0.908, as already mentioned on lines 391-392 of the submitted manuscript (note that the "final" configuration, from Table 7 is a little separate from the main part of this work so I will concentrate on the "best" configuration from Table 4, which is what Figure 5 refers to).

It is also worth restating that the achievement of the global parameter search was not so much to provide an overall improvement in fit to observations but to provide a more physically realistic underpinning for the simulations that should support future developments (e.g. the more physical cloud overlap, the use of a non-spherical particle for cloud ice for the first time, the use of ARTS database particles to support future sub-mm work)

2) I acknowledge that the "snow mixing ratio" dimension is not a physically-justified way to improve the results. However, it was never intended to build a fudge factor into the upgraded version of the forward model, but rather to use this as a straightforward way to investigate a number of interesting things: (i) it underpins Figure 9, which shows the stability of the results to potentially large errors in the modelled convective mixing ratio; (ii) it is also helpful in the situation-dependent parameter search (Table 6) and discussion (section 6.1). Here, the need to increase it in tropical convective areas to better fit the observations might indicate an error in the modelled convective mixing ratio, but it might also point to a need for broader convective cores, thicker anvils, or bigger and/or more scattering particles, as discussed in the text. Hence, the "snow mixing ratio" dimension is

an important part of the work and I would like to keep it. However, its purpose could be introduced better in section 3.2, where I will try to add a few sentences in the revised manuscript.

3) This comment suggests that one way to improve the representation of convective anvils could be to treat them as a microphysically distinct cloud type, separate from the large-scale ice cloud generated by frontal uplift; hence one practical solution could be to treat anvil cloud as a separate hydrometeor type. This is a good suggestion and will be added to the conclusion of the revised manuscript.