Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-1045-RC2, 2021

Atmospheric Chemistry and Physics Discussions

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Interactive comment

Interactive comment on "Microphysical Processes Producing High Ice Water Contents (HIWCs) in Tropical Convective Clouds during the HAIC-HIWC Field Campaign: Evaluation of Simulations Using Bulk Microphysical Schemes" *by* Yongjie Huang et al.

Anonymous Referee #1

Received and published: 7 January 2021

The paper reports on a regional simulation of an MCS where large ice water contents were observed. Four different microphysical schemes were compared to observations. The paper is clear and to the point. I think that only minor revisions are necessary before publishing.

This is obviously a nice test case and one that others may try to reproduce themselves. The observations are freely available, but will scripts to reproduce the sampling outlined

Printer-friendly version

Discussion paper

here also be available?

Is there an accepted definition for HIWC region. Can this be indicated and compared in some way to show that models have some skill at predicting these regions? Including something like this would link the results nicely to the operational motivation for this work.

line 55. Could refer to Keinert et al. who have carried out laboratory experiments to investigate droplet freezing/shattering in this temperature range. (https://www.researchgate.net/publication/343566907_Secondary_Ice_Production_upon_Freezing_of_Freely_Falling_Driz

line 159. Feel free to ignore this because there are always more that can be added to an intercomparison, but given the operational importance of HIWC events i'm surprised that the Thompson scheme was also not included in the mix - i believe it is operationally used (or was) in the NOAA RUC model.

line 182 - '...likely associated with..' - i think you should be able to say yes or no to this by inspecting era data rather than leaving it hanging.

line 189 'using the assumptions consistent' - does this include the shape of the psd or just the mass-size/density assumptions?

line 207-210. Should be careful to add that your statement is for this metric: BT. e.g. this means deep convective areas as defined by this BT metric are larger in MORR than.... You could define it by updraft or specific humidity...

line 230. it may be too messy but it might be worth trying to add the cumulative frequency contours from the obs to the model panels to provide an easier way to compare across?

line 250. even though there is a bias in sampling - is this not compensated with the BT sampling methodology?

line 252. can you estimate the impact? If not i think you have to assume its unbiased...?

line 284. how is psd spread quantified?

line 302. reasonably to within x% ?

line 311. Possibly add a radar weighted mean size to link to radar results?

line 333. 'it is found that the main microphysical process rates at -45 and -30C are the same a those within profiles containing HIWC regions at -10C'. I don't think you mean this but i'm interpreting it as using process rates at -10C as a proxy for what is going on at -45 and -30C. If so, then i would expect processes involving graupel production to be different between -10C and -45C.

Additionally, in strong convection, the loss of liquid at lower levels controls the liquid being transported to higher up and the eventual anvil evolution.

At -45C the freezing is dominated by homogeneous freezing, whereas at -10C it will be heterogeneous freezing or secondary ice production. Therefore i don't think you can use the process rates at -10C as proxy for -45C.

line 335. i think i disagree here. Transporting more cloud droplets to homogeneous freezing altitudes/temperatures could lead to more numerous small ice crystals.

line 349-350 '...substantially underpredict the ice particle number for 0.1 mm < Dmax < 3 mm and overpredict the vertical motion in the HIWC regions, which results in stronger and higher-extended simulated radar reflectivity...' and line 355-356 '...an underestimate of ice particle number concentration, especially graupel, leads to large reflectivities...'

Because the psd is a gamma or exponential distribution in the model i accept that reducing total number concentration will lead to an increase in the reflectivity for a fixed mass. But invoking that strong correlation between number concentration dominated by the small end of the PSD and the large end of the PSD that affects the radar reflectivity is not a given for the real world.

In the real world, underpredicting number concentration will not necessarily be the direct source of the radar reflectivity overestimate - its the SUM_i(m_iˆ2) integral where i is the i_th size bin. If the total mass is correct the overestimate of Ze must come from the the mass being in too large size bins as your PSD comparisons suggest.

I can see that in the appendix the radar reflectivity is formulated to depend upon Nt due to the gamma assumption, but it feels more physical to relate the effect to the large end of the PSD.

line 354 - do you need to add a total ice category line to the MORR plot to compare to P3?

line 371. you could resample the data to match the liquid water content from the model and observations and then see if the ice properties etc are biased.

line 375. ice nucleation = homogeneous freezing? or hom+het freezing?

line 378. i could not really see this figure - all i can see is the total ni_tend going out of range. It looks like a rime splintering secondary ice production is represented but has no effect?

line 451. the 3km radar results should appear earlier than the final page i think.

line 459 it doesnt look that clear cut to me. based on red triangles in fig 11 i score it as p3-2ice=2, wsm6=1, morr=1

The psds using the mean of the De metric: -10C wsm6, morr, p3_1, p3_2 -30C p3_1, morr, wsm6, p3_2 -45C p3_1, morr, wsm6, p3_2

cfads - the cumulative curves from p3_1 seem to match best with obs. then wsm6, morr, p3_2

If you are going to say which is best i think you need some quanitative measures to quote.

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