

Atmos. Chem. Phys. Discuss., referee comment RC3
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Comment on acp-2022-670

Anonymous Referee #2

Referee comment on "Sensitivity of convectively driven tropical tropopause cirrus properties to ice habits in high-resolution simulations" by Fayçal Lamraoui et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2022-670-RC3>, 2022

In this article the authors describe the importance of accurately simulating TTL cirrus. Specifically, they compare three different microphysics schemes and show that only the one that accounts for ice crystal habit accurately simulates the observed high IWC (HIWC) found in the StratoClim measurements. They also show that this scheme can simulate the large aggregates found in the measurements. This is important work and falls well within the scope of ACP. However, I find some of their arguments tenuous based on how they present their study (mostly writing and figures). Overall, I recommend publication, but after major revisions. I hope my specific comments and questions below help to clarify and strengthen this study.

Line 46: Can the authors clarify the sentence starting with "Therefore, air injected into the lower stratosphere..."? Based on the writing I am not sure how the previous sentences result in this statement. Also, "than into the troposphere" should be "than in the troposphere".

Line 48: is there a reference to explain the "tropically averaged level of all-sky zero net radiative heating"?

Line 61: please clarify "cold trap". Is this the CPT? Also, delete "On the other hand" at the beginning of the sentence.

Line 63: This should be the start of a new paragraph. I can't tell if that is the authors' intention in this template. If so, then ignore this comment.

Line 66: the authors could expand this to include other estimates. Satellite based estimates by Sassen and L'Ecuyer place cirrus coverage much lower than what is quoted here.

Line 68: GCMs acronym is never defined

Line 130: "(Fig. 1)" should refer to Fig. 3 instead.

Line 136: the statement starting with "Beyond that, it increases...". Please add a reference to Figure 4

Line 139: "D01" is never explicitly defined in the text

Line 157: is the New Tiedtke Scheme enabled for the outer domain only due to horizontal resolution? I assume the inner domain is convection resolving

Paragraph beginning on Line 160: please more clearly define pre-defined ice categories for readers

Line 169: delete "problematic" and describe this more clearly why artificial conversion rates are unphysical

Line 175: why do the m-D and vt-D relationships not properly capture the transitions in ice particle mass and fall speed during riming?

Line 198: please explain what is meant by the 99th percentile and how this is calculated. Also, can the authors explain why they chose the 99th percentile? Why not the 75th percentile?

Line 202: add "value" to the end of the sentence

Line 205: The statement beginning with "to examine the sensitivity..." includes a lot of repeated information from the methods, please reword

Line 229: please reword "Below this line, ice particles are associated with weak updrafts and represent mostly in-situ cirrus clouds that are likely heterogeneously, or, less likely, homogeneously.". The latter half of the statement should read something like, "...in-situ cirrus clouds that likely originate from heterogeneous nucleation, and less likely form homogeneous nucleation."

Line 246: this is not clear based on Figure 12

Line 266: again, the 99th percentile should be defined somewhere in the text, in line with my previous comment. The figure does not show the 99th percentile of the data for the measurements or any of the simulations except for the Thompson scheme. I would also say that all simulations overestimate smaller ice crystals compared to the observations. See comments on Figure 15 below. Finally, why does the Thompson scheme stand out from the others between 10-15 μm .

Line 315: please add a reference to figure 14 after "due to the difference in RHi"

Line 391: isn't the statement "the low rate of in-situ nucleation" contradictory of the statement above saying that the PSD indicates more in-situ nucleation of planar ice crystals in the anvil?

Line 330: shouldn't "scatter plots of RHi as a function of fall speed" be "fall speed as a function of RHi"? Also, it is unclear why this connection is being made. These are seemingly unrelated quantities. Can the authors add some explanation perhaps?

Line 390: why does adding more realistic and complex ice habits alter the radiative heating of clouds? Please elaborate in the text to make it clear for the reader.

Line 419: "As the properties of cirrus clouds may differ depending on the geographical location and proximity to convections, this study is associated with a deep convection case during Asian summer monsoon." Please reword this statement as the first clause does not motivate the second. Also, add an article before "Asian summer monsoon".

Line 427: I don't think the sensitivity to grid spacing was really that clear from the figures. Really, what is more important is the difference between the three schemes. This is in line with my comments of Figure 7, 8, and 9 below.

Line 433: change "vertical grid spacing" to "vertical resolution" if you want to keep this sentence. Otherwise, it reads as random and out of place relative to the rest of the paragraph. Consider placing it somewhere else.

Figures

General comment on all figures: those with subplots should include a label (a, b, c...) to help distinguish them within the main text. In some figures you refer to the Jensen scheme and other you label it as Ishmael. Please use a consistent label. Moreover, please chose a different colormap than Jet where applicable. This is not colorblind friendly, it introduces bias in interpretation of the results, and in some cases the reader cannot distinguish between the modelled data and the observations as they are plotted in the same color (e.g., Figure 10).

Figure 1: this is a great figure. Though, please describe everything in the figure in the caption. For example, next to the arrows are the altitude of the aircraft and the distance, which I assume is the distance from the origin, but I can't tell for sure.

Figure 4: this is a confusing figure as it looks very much like a tephigram. The caption should be much more descriptive, stating something along the lines of vertical resolution as a function of model vertical level. Also include a description of the shaded area. Also, do not refer to it as "vertical spatial resolution" as spatial indicates a single level and may confuse readers.

Figure 7, 8, and 9: I am not these three figures and the associated discussion really portray the message the authors would like to convey, which is to show sensitivity to model domain. I am not sure this is really needed for this study. I would rather show the differences in the inner domain between the three microphysics schemes in one figure and move these three figures as they are now to an Appendix.

Figure 14: please move the legend below and make it larger so it is easier to read.

Figure 15: I would show the full range to be in line with the discussion on Line 266 and then perhaps place a box over the region of interest (0-50 μm) and shown a blow up of that range as a subplot.

Figure 19: can the authors add fit lines to each subplot to aid their discussion in the paragraph starting on line 357?

Minor comments

Sometimes you are missing an article in cases where you need one. For example, on Line 442 "and Ishmael scheme..." should read "and the Ishmael scheme...". Please review all instances of this.

Line 418 change "fewer" to "few"

Line 419 change "significantly" to "frequently"

Line 373: delete "the" before "controlling"

Line 458: make hydration lowercase.

