Review of Gasparini et al., submitted to ACP

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Title: Opinion: Tropical cirrus—From micro-scale processes to climate-scale impacts

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General comments:

The authors review the recent literature on tropical cirrus and expose their perspective as to where the field is going, which challenges lie ahead, and which important questions must be answered to. Observational, theoretical, and modeling aspects are addressed. Both convectively generated anvil cirrus and in situ generated TTL cirrus are discussed. An important aspect emphasized throughout the review is the very wide range of scales over which the underlying physics of cirrus happens, from ice microphysics to cloud macrophysics. In light of such scale disparity, the authors discuss the new opportunities offered by the next generation of global storm-resolving models, but also new challenges that come with operating at the cloud-resolving scale. The impact of new observational capacities expected to become available in the next decade is also discussed.

I generally like and support this review. Given how uncertainties linked to tropical cirrus are pervasive in many aspects of climate projections, I think it is useful at that stage to reflect upon the progress that has been made in the field and outline the directions of research that should be prioritized. This work is also timely. As noted by the authors, we are at a crossroad of traditional approaches and new approaches fueled by rapid increase in computational and observational capacities. Metrics for measuring progress may have to be redefined and challenges of consistency may require in some cases to rebuild from scratch rather than attempt to unify new developments with already existing parameterizations.

Given that this review is basically an opinion piece, it entails a degree of arbitrariness in the presentation of the material that reflects the choice of the authors and that I will not question. I generally agree with the structure of the manuscript and classify my recommendation as minor revision. However, I have some specific and minor comments, outlined below, that I encourage the authors to address.

Specific comments:

One point that I wish the authors had featured more prominently is the added value of water isotopologues for cirrus studies. I suggest that the authors add a few sentences to this regard, in the manner they will find appropriate. I know isotopes are mentioned at line 140 but almost in passing. Isotope packages have been embedded in cloud-resolving models and featured in a number of recent studies. Efforts are also underway in Japan to bring isotopes to the NICAM GSRM. This seems relevant for the scope of this review. In parallel, laboratory measurements of isotope fractionation factors have tested the importance of surface kinetic effects for ice growth at cirrus-relevant temperatures, which is relevant to sections of the manuscript where the deposition coefficients for ice growth are discussed.

I suggest that the laboratory work on ice deposition [1] [2] be referenced near line 580. (See corresponding references at the end of this document.) You could mention for instance that the alteration of the preferential partitioning of water isotopologues by non-equilibrium kinetic effects at ice deposition can inform on surface kinetics and deposition coefficients.

Moreover, Section 6.3.3 (Passive tracers) seems a good place to reference isotopic studies with isotope-embedded CRMs. You could for instance present water isotopologues as a kind of tracer of water vapor and ice pathways in the atmosphere, and mention the modeling studies [3] [4].

Unrelated to this, I have another comment on the concept of supersaturation in the TTL. In Section 4, near line 420, you write: "The fundamental reason why cirrus occur so frequently near the tropical tropopause is the prevailing upward motion that drives supersaturation and cloud formation". I think this is misleading, if not erroneous. Earlier TTL work (e.g. Sherwood and Dessler 2020) attempted precisely to explain why there is no widespread vapor saturation at the mean tropopause temperature despite tropical mean upward motion. The consensus, as you correctly state near line 421, involves lateral transport through cold traps, as first formalized by Holton and Gettelman 2001 and further confirmed by subsequent Lagrangian studies. I therefore suggest that you modify your sentence accordingly and emphasize the Lagrangian cold traps instead of vertical motions in the TTL.

Minor comments:

Line 1: "Tropical cirrus clouds". I think you should define cirrus here in the abstract for the broader audience: "Tropical cirrus clouds, i.e. high-level ice clouds, play a critical role […]"

Line 13: Typo?. "understanding" -> "understand"

Line 21: Perhaps add "in situ ice formation outside of convective clouds" to show that you rule out in situ formation within anvils here?

Line 39: I have difficulty parsing the beginning of that sentence. Perhaps remove the comma: "While thin in situ cirrus in the TTL […]"

Line 82: Typo. "currently currently". Remove extra occurrence.

Line 130: You may also want to mention CR-AVE in 2006, which paved the way for TC4.

Line 142: Typo. "retreival" -> "retrieval"

Line 188: "GSRMs represent deep convection explicitly". I would prefer "GSRMs resolve deep convection explicitly".

Line 247: Typo. "at the top of atmosphereor surface" -> "at the top of atmosphere or surface"

Line 249: You may want to reformulate "Modeling studies show that the ice microphysics scheme can alter top-of-atmosphere outgoing longwave radiation […]" to show that you place yourself in the realm of models here.

Lines 260-261: Same. Suggestion of reformulation: “But cloud radiative heating, especially in the upper troposphere, has a more direct influence on circulation, and modeling studies show a strong modulation by ice microphysical factors such as the aerosol dependence of the nucleation scheme […]”

Line 397: Maybe remove “like” and reformulate “This seems a likely possibility […]”?

Line 418: Besides climate impacts, you may want to mention that changes in stratospheric humidity can also impact stratospheric ozone chemistry [5].

Line 419-420: See specific comments.

Line 425 (near): You may also want to mention the seasonal cycle in TTL cirrus here, that follows the seasonal cycles in tropopause temperature and Brewer-Dobson circulation.

Line 460: Maybe you can mention the seasonal cycle in tropopause temperature again here, besides the temperature variability driven by QBO and ENSO.

Line 476: Perhaps you could add “since vapor pressure adjustment is much faster over liquid water than over ice.” after “liquid water clouds” and cite Korolev and Mazin 2003 [6] here. This makes the argument easier to follow.

Line 476 (bis): Typo. Delete comma after “both”

Line 510: Typo. “challenges” -> “challenge”

Line 557: I am not sure I understand the word “surprising” in this context. Did you mean “unsurprising” instead?

Line 561: I think you should remove “the last steps before ice nucleation in cirrus formation” or reformulate as I am not sure what is meant here.

Line 561 (bis): On the particular issue of ice formation in turbulent flows that is raised here, one possibility is to build upon the work on droplet growth in isotropic turbulence (e.g. Lanotte et al 2008 [7]). You could mention that here.

Line 564: You should add “vapor **and heat** diffusion through the air […]” as the necessity to extract latent heat across the diffusive boundary layer surrounding crystals creates thermal impedance on ice growth.

Line 579: I would also cite Lamb et al 2017 here [2].

Line 680: Typo. “schemes” -> “scheme”

Line 685-693: See my specific comments. This may be a place to mention that water isotopologues have been used as a tracer of the pathways of vapor and moisture and reference modeling studies.

Line 736: I think you should define “lidar ratio” (extinction to backscatter coefficient) here as one cannot assume that every reader will be familiar with this concept.

Line 766: Did you mean “GSRM” instead of “SRM”?

Line 781: Typo. Add space between Held (2005) and Jeevanjee et al. (2017)

Suggested references:

[1] J. Nelson, “Theory of isotopic fractionation on facetted ice crystals,” *Atmospheric Chem. Phys.*, vol. 11, no. 22, pp. 11351–11360, 2011, doi: 10.5194/acp-11-11351-2011.

[2] K. D. Lamb *et al.*, “Laboratory measurements of HDO/H2O isotopic fractionation during ice deposition in simulated cirrus clouds,” *Proc. Natl. Acad. Sci. U. S. A.*, vol. 114, no. 22, pp. 5612–5617, May 2017, doi: 10.1073/pnas.1618374114.

[3] P. N. Blossey, Z. Kuang, and D. M. Romps, “Isotopic composition of water in the tropical tropopause layer in cloud-resolving simulations of an idealized tropical circulation,” *J. Geophys. Res. Atmospheres*, vol. 115, no. 24, Dec. 2010, doi: 10.1029/2010JD014554.

[4] A. J. de Vries, F. Aemisegger, S. Pfahl, and H. Wernli, “Stable water isotope signals in tropical ice clouds in the West African monsoon simulated with a regional convection-permitting model,” *Atmospheric Chem. Phys.*, vol. 22, no. 13, pp. 8863–8895, Jul. 2022, doi: 10.5194/acp-22-8863-2022.

[5] D. T. Shindell, “Climate and ozone response to increased stratospheric water vapor,” *Geophys. Res. Lett.*, vol. 28, no. 8, pp. 1551–1554, 2001, doi: 10.1029/1999GL011197.

[6] A. V. Korolev and I. P. Mazin, “Supersaturation of water vapor in clouds,” *J. Atmospheric Sci.*, vol. 60, no. 24, pp. 2957–2974, 2003, doi: 10.1175/1520-0469(2003)060<2957:SOWVIC>2.0.CO;2.

[7] A. S. Lanotte, A. Seminara, and F. Toschi, “Cloud droplet growth by condensation in homogeneous isotropic turbulence,” *J. Atmospheric Sci.*, vol. 66, no. 6, pp. 1685–1697, 2009, doi: 10.1175/2008JAS2864.1.