ACP review of Tully et al. (2022), 2nd review

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Title: Cirrus cloud thinning using a more physically-based ice microphysics scheme in the ECHAM-HAM GCM
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General Comments:

As I understand the reasoning in the rebuttal, since the orographic component of vertical motions resulted in modeled ice crystal number concentrations (ICNC) that substantially exceeded ICNC from in situ measurements and satellite retrievals, the orographic component was not used in order to achieve better agreement with these observations. When doing model development work, it is natural to strive to narrow the gap between observations and predictions for a given prognostic quantity, such as ICNC. Ideally, this gap narrowing is achieved by improving the model physics, but the practice adopted here for narrowing this gap is to remove a process of known importance to the ICNC; the process producing orographic gravity waves. The justification given is that homogeneous ice nucleation (henceforth hom) in this version of the ECHAM-HAM model is already an important process affecting ICNC within in situ cirrus when using the P3 cloud microphysics scheme. That's great, but that does not provide a rationale for removing the orographic gravity wave component of predicted vertical motions.

That orographic gravity waves are important to ICNC are not unique to Mitchell et al. (2016, 2018); this subject is also discussed in Gryspeerdt et al. (2018, ACP; in reference to their Fig. 1) regarding the satellite retrieval discussed in this rebuttal. That is, the DARDAR results reported in Fig. 1 of Gryspeerdt et al. (2018, ACP) clearly show a strong enhancement of ICNC over mountainous terrain outside the tropics, as do the results presented in Fig. 17 of Mitchell et al. (2018, ACP) and in Mitchell et al. (2016, ACPD) and Mitchell et al. (2020, ACPD). This can also be inferred from Figs. 4 and 7 in Barahona et al. (2017, Nature) as discussed in my first review. Moreover, cirrus cloud fraction is associated with orographic gravity waves as is evident in Fig. 4 of the satellite remote sensing study by Matus and L'Ecuyer (2017, JGR). As mentioned in all but the last of these studies, this is likely due to stronger vertical motions over mountainous terrain where the RHi threshold for hom can be achieved. The fact that ICNC retrieval methods indicate a strong ICNC dependence on orographic waves should motivate the modeling community to include the orographic component of vertical motions, regardless of how it affects ICNC in model simulations. If there is a gap between predictions and observations, try to improve some of the physics, but don't discard it. CCT can only be properly evaluated when all relevant processes are included, and orographic effects appear to be primary in importance.

Another point is to consider the subtle aspects of what determines ICNC in the in situ environment vs. the modeled environment. This was done in Appendix B of Mitchell et al. (2020, ACPD), where ICNC was calculated from the cirrus climatology of Kramer et al. (2020) using the reported in situ ice water contents (IWC) and mean volume ice radius r_{ice}, assuming an exponential ice particle size distribution (PSD) as often assumed in a climate model. With this PSD shape constraint, this calculated ICNC was often a factor of 2 or more greater relative to the in situ ICNC reported in Kramer et al. (2020) between 185 K and 220 K, although the calculated and in situ ICNCs were in general agreement between 220 K and 244 K. This may help explain the results under P3 Oro in Fig. 1 of this rebuttal.

Recalling that radiation transfer through clouds is determined by effective diameter D_e and IWC, and not ICNC, more emphasis on D_e (relative to ICNC) would seem appropriate. Note that two PSDs can have the same D_e and IWC while their ICNCs differ considerably.

Specific Comments:

Under conclusions, it is stated: “Such wide differences can be partially attributed to a lack of reliable in-situ observations of cirrus in order to constrain models, though this gap is starting to be closed with more recent studies (Krämer et al., 2016; Krämer et al., 2020).” Since cloud optical properties depend only on D_e and IWC in climate models, it is not clear how the in situ measurements in Krämer et al. (2020) will constrain these optical properties since only the mean volume ice particle radius, R_v, is reported in Krämer et al. (2020). We have modeled the relationship between R_v and D_e for gamma PSDs having different values of the PSD dispersion parameter ν, and for a given R_v, a wide variety of D_e are possible, depending on the value of ν. This is something this ETH group can easily verify. Note that there is no information concerning ν in Krämer et al. (2020).

Given the arguments under General Comments, regrettably, I do not understand how the conclusions in this manuscript can be justified.

David Mitchell