Review of “An Estimate of Global, Regional and Seasonal Cirrus Cloud Radiative Effects Contributed by Homogeneous Ice Nucleation”, by David L. Mitchell, John Mejia, Anne Garnier, Yuta Tomii, Martina Krämer, and Farnaz Hosseinpour, acp-2020-846.

Is homogeneous ice nucleation important in in-situ generated cirrus (temperatures below -38^{0}C)? Here's my thought on the question. Yes, in orographic wave clouds, in gravity waves, and in small convective cells that lead to cirrus uncinus clouds. Is homogeneous ice nucleation important globally? How would one address this question?

This study, which is densely packed and comprehensive, attempts to address this question through use of CALIOP lidar data and the using 40-year simulations from the NCAR Whole Atmosphere Community Climate Model version 6 (WACCM6). The model microphysics used is version 2 of the Morrison-Gettelman scheme (MG2). The effective diameter used as a reference is from the lidar. To estimate the heterogeneous (het) component, a somewhat questionable criteria is used. The CALIPSO De – T relationships obtained from the tropics are applied globally. For a given T, the De for latitude zones 0 – 30 N and 0 – 30 S are averaged for each season, and the resulting look up tables for ± 30° latitude are applied to all six 30° latitude zones. The assumed that because effective diameters were highest and concentrations lowest in this region compared to outside of it, the averaged values for this region would be most representative of het nucleation. My question is whether observations of in-situ cirrus in this zone support this view. I would like the authors to address this question. Possibly another way to do this would be to turn off homogeneous ice nucleation (hom) in the model and to assess the resulting changes. I look at the MG and MG2 microphysics articles and it was unclear to me exactly how hom was calculated. Although I don't know a better way for the authors to have approached this problem, I do question the reliability of the results. I do like the CALCAL method, which uses the CALIPSO De retrievals to scale the WACCM6 model.

Three other significant points I'd like to mention. First, in Sections 2.1-2.3, why are these formulations necessary-why couldn't they just be derived from the SPARTICUS/TC4 data sets? Secondly and more significant, the data sets you use for much of the analysis are extremely limited geographically and seasonally. Why not use the Kramer et al. (2020) for your analysis? Lastly, might it be possible to add a figure that graphically shows the results from Figure 10-16?

Other points of mention:

Page 1, lines 10 Specifically mention “in situ generated cirrus”

1, 10-11 How about anvils that shear off-they are likely the result of ice crystals nucleated at warmer temperatures?

2, line 2 In-situ generated

2, 13-14. How is De derived from CALIPSO? Brief mention of it would be useful, and perhaps an estimate of the error.

5, Eq (7). Why not do this directly from the data?

Eq. (17) I can see why you would like to use c and d based on Mitchell and Heymsfield but I suggest using the more recent values derived by Heymsfield and Westbrook (2010). I see you mention the differences later but wonder why your choice.

8, 25. But Vn compares poorly between this scheme and Heymsfield and Westbrook (2010). Perhaps this should be mentioned.

Eq. (25). Can't Dmean be expressed as a function of the IWC?

9, paragraph beginning on line 24. I'm concerned that the Dmean values of 50 microns have serious errors, because the 2DS probe data shouldn't really be used below about 30 microns, and so the estimates are likely in error because all of the 2DS sizes are used to derive Dmean. I do see that you also do the calculations for Dmean beginning in the second bin but still there are some questions that I have about the uncertainty (depth of field, etc.)

10, Section 2.5. I really like the method used for smoothing.

11, 12. I thought an optical depth range up to 2.0 was valid but not to 3.0.

12, 28. Sourdeval et al. (2018) in their Part I use a much larger data base than you use. They use COALESC, ML-CIRRUS , ACRIDICON-CHUVA, ATTREX-2014 and SPARTICUS. I wish your data set was not based only on SPARTICUS and TC4.

12, 30. Any high RHi region would be short-lived. Can this really be concluded from satellite-based retrievals?

12, 31. After doing digging, I went to the Petzold et al. (2020) article. They used MOZAIC (1994–2010) data as well as other data sets. You should mention that these RHi were derived from measurements on commercial aircraft.

15, lines 20-27. How much is CALCAL in the tropics due to deep convection?

16, lines 2-7. The problem with CALCAL in the mixed-phase zone is that it's unreliable because the CALIOP lidar is probably partially or fully occulted in that zone.

18, 25-27. Are there indications from in-situ data that support the view that high ice concentrations (10's per cm3) are found in updraft regions of cirrus (not mountain waves) at temperatures below -38C at which hom might be expected?