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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "CALIPSO observations of the dependence of homo- and heterogeneous ice nucleation in cirrus clouds on latitude, season and surface condition" *by* David L. Mitchell et al.

Anonymous Referee #2

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The authors propose a novel way, using combined lidar and spectral IR space observations, in synergy with relationships of physical variables obtained from airborne measurements, to obtain ice crystal number consentrations of single-layer cirrus clouds within a range of optical depth between 0.3 and 3. These results may be of great interest and relevance, yet at present they raise a number of very serious concerns, and the results presented are overinterpreted (see major concerns below). Hence we advise major revisions.

Major concerns: 1.a. The selection criteria (T<235K, OD between 0.3 and 3, radiative contrast with surface > 20K, single layer) lead to only 2% of sampled cirrus frequency map statistics appears quite noisy. Why did the authors only analyze 2 years of data,

when much more data are availabe?

1.b. The range of optical depths considered ranges from 0.3 to 3, so very different clouds are mixed together. It is necessary to distinguish between optically thinner and thicker cirrus.

2. The introduction clearly describes motivations related to in situ formed cirrus clouds. Yet it does not seem that the authors have a way to filter out anvil cirrus in their analysis. More generally, 'liquid origin' cirrus, whether originating from anvils or from warm conveyor belts, should be isolated.

3. Going from an estimate of the ice cystal number N to a conclusion on the nucleation is quite a step. Obtaining information on N is already very interesting. It would be better if the authors stick to results on N rather than try and push too far their interpretation. 4.a beta, which is related to De and N/IWC, is determined from 2 wavelengths. the IIR instrument has yet a third channel which should be more sensitive; why are not the 3 channels used, especially since you want to get the information on both parameters?

4.b you derive beta for OD down to 0.3; at this OD the contribution from the atmosphere is quite important which should introduce biases. Was the effect studied?

4.c The relation between cirrus properties and beta is obtained from in situ measurements from aircraft campaigns. How this applies to satellite observations which have a very different spatial footprint is not straightforward.

5. From 2 campaigns, and distinguishing fresh and aged cirrus in one, one sees that fresh anvil cirrus is beyond the sensitivity (beta < 1.1) and the other data which show indeed a spread over beta are incredibly close to the fit. It would be more convincing to have this fit made from several campaigns. Especially aged anvils are not really the focus of your analyses.

6. There are a number of assumptions that go into the derivation, e.g. the shape of the ice particles (hexagonal columns down to sizes where they are not truly relevant).

7. In polar locations, one has to be very careful with the underlying surface: the emissivity of ice, or sea ice (aging sea ice or fresh snow), has to be treated with special care. Was this taken into account for the retrieval?

8. There are many figures (22), many of them multi-panel figures (18 panels for each of the figures 15 to 18), adding up to more than 120 panels. This is too much. Surely it is the authors' responsibility to summarize and synthesize their work in a reasonnable number of figures.

9. There exist other retrievals of ice crystal number concentrations, using lidar - radar synergy, for example from DARDAR products (Delano and Hogan J. Geophys. Res. 2008 and 2010). How do your results compare to these ?

Minor points

p1, line 26: relevancy or relevance?

p2, lines 6-7: a reference justifying this assertion would be useful

p2, l14: Koop [et al], 2000

p2, l22: There is a recent study on gravity wave fluctuations and their implications on nucleation processes which provides an update and complement to the Haag et al (2003) reference: Jensen, E. J., et al. (2016), High-frequency gravity waves and homogeneous ice nucleation in tropical tropopause layer cirrus, Geophys. Res. Lett., 43, 6629–6635, doi:10.1002/2016GL069426.

p3, l4-6: 'they draw from a limited number of field campaigns... of het and hom cirrus.' This sentence is too critical of past field campaigns. Of course field campaigns have their limitations, and of course it is necessary to complement them with global observations, but these should be presented more as complementary, more positively.

p3, l16: I have not found the explanation in the text of the GMAO acronym.

p9, section 3.2: I found the presentation a little awkward: the full equation is given first

(equation (4)), then explanations for the different parts of the equation are provided. It is fine to stick with this, but I suggest below an alternative suggestion: - information on beta_eff allows to obtain N/IWC; hence to obtain N we wish to estimate IWC. This is given by (5) (a reference could be useful here, although this may seem standard). - now in this expression, it is needed to estimate the effective layer- mean extinction coefficient, and this is given in (6). - putting the pieces together, an expression for N can be obtained from CALIPSO IIR and CALIOP (for Delta z_{eq}): eq (4).

p11, l14: the OD range limits the conclusions of this study to visible cirrus; hence by construction the subvisible thin cirrus near the tropopause tropical layer are not covered.

p14, lines 26-27: why this choice of only two years, 2008 and 2013?

p15, l 28: It is inappropriate to include a figure which does not contain a color bar allowing to read the altitudes corresponding to the colors. Moreover, the figure contains more information than needed as the oceans' bathymetry is included. Finally, the purpose of the figure is only to recall the general location of Earth's major topographic features, and I believe most readers will be familiar enough with this (a detailed knowledge of topography is not required in the present study).

p16, l22: it is a bit surprising that het cirrus would be the most abundant over oceans, where ice nuclei concentrations may be expected to be weaker than over land. This is rather the signature that the intensities of updrafts matter more than the abundance of ice nuclei, is that the proper way to understand it,

p17: Figures 15-18: There are 72 panels in those figues. This is too much. I think the authors should identify patterns and show a more limited number of figures to display the main patterns, and add only those panels which contribute to interesting deviations from the general pattern. The authors should For instance, for 60S-30S, the four figures (15 to 18) show the same pattern, with variations that can be summarized in the text, or by a choice of fewer figures (just JJA and DJF). It is true that, with the figures available, one could spot small variations in the details; but at this stage, given the uncertainties which one should keep in mind for a new, innovative method of remote-sensing, one should give too much weight to the details of the distributions.

p17, l27-28: the statement seems a bit exagerated, pushing the evidence further than should be.

p18, l1: the precision is unnecessary, 0.43 +/- 0.05 seems sufficient, given the uncertainties.. This is true in many other places of the text, e.g. p20, lines 2 and 6....

p18, l20: it is very good that this is recalled, this should be emphasized also in other parts of the text, e.g. in the conclusion.

p20, l9-10: clouds up to 25km: are these Polar Stratospheric Clouds? In which case they should be distinguished from cirrus. Their physics are not the same. It would not make sense to mix the two populations.

p20, Figues 19 and 20: the maps are so patchy! It would be better to average together 2008 and 2013, to analyze more data (why just two years?) and probably to average zonally.

p22, line 2: reverse order: 'exhibit a hom-het annual cycle similar to ...'

p22, l25: I am not sure that 'implies' is the proper word here. Should this rather be 'This accounts for our understanding that in situ cirrus...'

p23, l8-9: This sentence is a bit mysterious. In particular, what is meant by 'mechanistic'?

p23, l22: any ideas for the reason of these differences?

p24, l20: change to a comma: 'wave resonance absorption, a process...'

p24, l20-22: this point will be more convincing if more campaigns are used to establish the relationship.

p24, l25-29: this is an interesting point.

p25, l15: split into 2 sentences, : ...solar zenith andgle. With the SW...'

p26: these last paragraphs on Arctic Amplification are too long given the speculative nature of the arguments at this point.

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