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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A 6-year global climatology of occurrence of upper-tropospheric ice supersaturation inferred from the Atmospheric Infrared Sounder after synergetic calibration with MOZAIC" *by* N. Lamquin et al.

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We copied the comments of referee 2, the replies are then indicated by –>

General Comments:
The manuscript is too long, and there are too many figures. Figures 2, 5, 9 could probably be eliminated. I also think that some of the maps (figures 10,11 for AIRS, 16, 17 IFS, Fig 20, 21 ECHAM) could be combined since there is little discussion. The 6

panel zonal mean figures (12, 18, 22) could be made into zonal mean lat-height plots , perhaps with values and then difference from AIRS.
–> we have removed some of the least significant figures and made lat-height plots for the zonal means.
On the whole there is too much method and not enough results and comparisons. There is little commentary on the comparisons at the end. There is also virtually no comment on how different the results are from previously published work with AIRS, and the raw AIRS data you are using.
–> we have changed the text
The aim of looking at cirrus clouds is not done that well in the manuscript, and I would probably recommend focusing on the vapor phase in this paper, and worring about cirrus in a different paper.
–> the main concern is the vapor phase, the comparison with CALIOP is only an illustration.
The error analysis for MOZAIC is pretty cursory and should be improved. Just saying there are differences between MOZAIC periods and this is a 'bias' is not appropriate.
–> see answers to questions below
Specific comments:
P12893, L8: Why not use the mixing ratio, rather than altitude as a threshold (i.e. H_2O < 20 is probably bad).
–> we agree that low vapor values are a concern, using a threshold to remove these values would bias the result even more. An altitude of 200 hPa is a realistic limitation to consider whether we can be confident in the final statistics.
P12895, L14: This only works if the variance is constant. What would happen if you used the mean? Does it change the answer?
–> the point is to detect ice supersaturation at least once within the AIRS FOV both vertically and horizontally, therefore we use the maximum to increase the chances of detecting it. The mean within the FOV would slightly lower the probability (about 5
P12896, L8: Are you trying to look for MOZAIC observations in cirrus observed from

satellite? Not much chance of this due to vertical uncertainties I would think. Yes there is higher MOZAIC RHi near clouds but I worry about sampling bias in MOZAIC. Have you considered that?

–> we have removed this part since we did not find significant changes in the RHi distributions whether MOZAIC were close or far from the cirrus clouds. The next paragraph has also been modified consequently.

P12896, L17: I do not think satellites can provide in Cirrus RHi.

–> the text have been adapted to emphasize the difference between the AIRS and the MOZAIC measurements in relationship with clouds

P12897, L15: Which period is biased? Why would more recent measurements be worse than earlier? This does not make sense. Could it be just sampling? I would probably trust the more recent period more.

–> there is a real calibration problem in the most recent period (Herman Smit personal communication added) P12897, L17: Not a 'bias'

–> changed into 'shift'

P12897, L22: This uncertainty analysis is just waving hands and needs to be made more rigorous.

–> the original uncertainty is in Gierens et al. (1999) for the absolute uncertainty of RHim (their figure 1) and the shift is regionally consistent. There is no possibiity of going further into it.

P12899, L29: What is the purpose of the estimate? If the purpose is to understand when clouds might form, then perhaps the just once approach is best (radiatively, any cloud is what you want).

–> the purpose is only the detection of ISS, whatever thin or thick is the supersaturated layer.

P12900, L11: Why is a hyperbolic tangent justified? I do not see anything supporting it here. Is it previous work? Dickson et al 2010? How sensitive are the results to choice of function?

–> we added details in the text, the hyperbolic tangent best fits the points which are

shown on the figures, out of 100

P12901, L15: But this is a real effect of cloudiness: it indicates higher humidity. It doesn't seem to be a bias to me.

–> we agree it is a difficult point to assess but cloudiness has also a reverse effect on the AIRS RHi: if cloudiness is "greater" then RHi_A gets higher which thus contributes in an opposite effect in the figure. Therefore we suspect the observed effect is more a matter of vertically matching the ice supersaturated portions of the atmosphere.

P12904, L28: Here is a case where the correction needs to be compared to the Raw AIRS data: both published, and based on the raw AIRS data you have.

–> we agree that a comparison with Gettelman et al. (2006) is both necessary and interesting, we added a paragraph.

P12906, L14: In Figure 13 you are not using an independent data set. So this does not validate data out of the regions with MOZAIC data. Dynamics maybe different. For example, there are observations that Hemispheric ice supersaturation thresholds are different (J. Ovarlez, J. F. Gayet, K. Gierens, J. Strom, H. Ovarlez, F. Auriol, R. Busen, and U. Schumann. Water vapor measurements inside cirrus clouds in northern and southern hemispheres during INCA. Geophys. Res. Lett., 29(16), 2002.) Please do an independent validation (i.e., validate with different years of MOZAIC data).

–> instead of 'evaluation' it is more appropriate to refer to it by 'comparison'. AIRS and MOZAIC are used independently, also other years of MOZAIC data lead to similar comparisons but we cannot do anymore comparisons.

L12908, L11: Several other models have comprehensive ice supersaturation: A. Gettelman, X. Liu, S. J. Ghan, H. Morrison, S. Park, A. J. Conley, S.A. Klein, J. Boyle, D. L. Mitchell, and J.-L. F. Li. Global simulations of ice nucleation and ice supersaturation with an improved cloud scheme in the community atmosphere model. J. Geophys. Res., 115(D18216), 2010. M. Salzmann, Y. Ming, J.-C. Golaz, P. A. Ginoux, H. Morrison, A. Gettelman, M. Krämer, and L. J. Donner. Two-moment bulk stratiform cloud microphysics in the gfdl am3 gcm: description, evaluation, and sensitivity tests. Atmos. Chem. Phys., 10(16):8037-8064, 2010.

–> we added references and changed the text accordingly
L12909, L19: Why not just a single level around the flight altitude
–> because flight altitudes are randomly distributed within the layers, we use AIRS data independently (no collocated MOZAIC data)
L12909, L20: State season on figures 10,11,16,17
–> added on the figures
L12910, L21: Figure 19 could be eliminated.
–> we have made the appropriate changes
L12911, L13: Why should supersaturation be maximum random? I would think random (in the UTLS) might be better.
–> neither for natural clouds nor for ISS areas it is clear which overlap scheme would be most realistic. The truth is likely to lie between maximum random and random overlap but maximum random overlap is assumed to be consistent with the overlap scheme for clouds.
L12911, L16: Describe the season better in the caption.
–> added on the figures
L12912, L26: Might be good to put IFS and ECHAM together on some plots if lines, or do as lat-height with difference from AIRS as suggested if you want to keep the vertical.
–> we have made appropriate changes
L12913, L12: Why does the weibull distribution matter? L12913, L28: "good" is vague. Rephrase.
–> we have removed the comment on the Weibull distribution and changed the text

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Interactive comment on Atmos. Chem. Phys. Discuss., 11, 12889, 2011.