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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.

Anonymous Referee #1

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

This manuscript describes a method for adjusting AIRS ice supersaturation observations by correlations with MOZAIC aircraft data, and comparing it to a forecast model and a climate model. The paper is generally well written, but far too long, with too many figures. With major revisions, it should be suitable for publication in Atmospheric Chemistry and Physics.

General Comments:

The manuscript is too long, and there are too many figures. Figures 2, 5, 9 could probably be eliminated. Figure 6 and 7 seem pretty duplicative. 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.

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.

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 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.

Specific comments:

P12893, L8: Why not use the mixing ratio, rather than altitude as a threshold (i.e. $H2O < 20$ is probably bad).

P12895, L14: This only works if the variance is constant. What would happen if you used the mean? Does it change the answer?

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?

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

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.

P12897, L17: Not a 'bias'

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

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).

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

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

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.

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).

L12908, L11: Several other models have comprehensive ice supersaturation:

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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.

L12909, L19: Why not just a single level around the flight altitude

L12909, L20: State season on figures 10,11,16,17

L12910, L21: Figure 19 could be eliminated.

L12911, L13: Why should supersaturation be maximum random? I would think random (in the UTLS) might be better.

L12911, L16: Describe the season better in the caption.

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.

L12913, L12: Why does the weibull distribution matter?

L12913, L28: "good" is vague. Rephrase.

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

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