Atmos. Chem. Phys. Discuss., 8, S8430–S8433, 2008
www.atmos-chem-phys-discuss.net/8/S8430/2008/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evaluation of upper tropospheric humidity forecasts from ECMWF using AIRS and CALIPSO data" *by* N. Lamquin et al.

Anonymous Referee #1

Received and published: 21 October 2008

Review of "Evaluation of upper tropospheric humidity forecasts from ECMWF using AIRS and CALIPSO data" by Lamquin, Gierens, Stuenrauch, and Chatterjee.

General Comments:

This manuscript evaluates the ECMWF ability to forecast upper tropospheric humidity relative to AIRS relative humidity (RH) fields and CALIPSO cloud occurrence. Since improving the link between ice supersaturation and cirrus occurrence in forecast and climate models is critical to improving the prediction of cirrus and their radiative impacts, this type of comparison is warranted. Overall, this manuscript is well written and concise, the scientific method is generally clear (with some exceptions listed under

Specific Comments), and the conclusions are clear.

While satellite data is probably the most convenient way to assess the global or region distribution of ice supersaturation, I have some concerns about its use without clearly stating the uncertainties. In reference to the use of AIRS data, the authors spend quite a bit of time discussing the co-location of satellite instruments, but there is very little text provided concerning the uncertainty and evaluation of the AIRS humidity fields. This is probably mentioned in some of the cited references, but it would be helpful to summarize the salient points. In order for the reader to assess this comparison, the AIRS RH (and RHI) uncertainty needs to be quantified and discussed in terms of the authors conclusions. My second concern is in how the RH with respect to ice (RHi) is computed. There is no mention of the formulas used to compute saturation vapor pressure (see specific comments below). This is critical in comparisons of ice supersaturation from distinctly different sources. Different formulas have very different saturation vapor pressures, particularly at cold temperatures. Please state and discuss the calculation of RHi from both ECMWF and AIRS. If the AIRS and ECMWF use different formulas, how does the uncertainty change your conclusions?

Overall, I think that this manuscript should be published after the following comments are addressed and specified revisions are performed.

Specific Comments:

Fig. 1: it is unclear what the resolution of the AIRS data is. Please make this clearer in Fig. 1 and in the text.

Sec. 2.2. In this discussion of different data products, I began to wonder what you are going to use all this data for. It might be useful at the end of each section or at the end of each instrument discussion you put a statement explaining what the data will be used for in your analysis. This will build a roadmap for the reader.

Figure 5. I find the discussion of Fig. 5 on P. 17916 to be a bit confusing. It might help

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if you label the individual figures with letters (a, b, c, etc.) and then refer specifically in the text to those figures (i.e. Fig. 5a). Often I did not know which figure you were discussing. Also, I don't really find a discussion of the far right column figures in Fig 5 (where you take the RHI difference).

Throughout the manuscript, you refer to the lidar measurements aboard the CALIPSO satellite as CALIPSO. I believe the correct name for the lidar is CALIOP.

P. 17917, Lines 9-12. Can you explain why you see this connection between the IFS humidity around ice saturation and the CALIOP and AIRS measurements? Does the IFS output in this context contain both clear and cloudy points, or is this information not captured in the IFS output? Would the connection be the same for clear, clear+cloudy, and cloud only conditions?

P. 17918, Sec 4.2. Can you describe the horizontal size of the ECMWF IFS grid box relative to the CALIOP footprint? Are they similar?

P. 17918, Lines 20-22. Do you have a reference for the homogeneous nucleation threshold that you are citing?

P. 17919, Lines 3-9. It seems that the majority of the points in Fig. 9 (bottom left) are within 100 hPa of one another. Can you state approximately what 100 and 200 hPa equates to in physical thickness?

P. 17919, Line 13, I think that the bottom right plot in Figure 9 is "right-skewed" not "left-skewed".

Figure 10. You might want to label the x-axis on the top two figures as RHi_E so that it is clear that it is from ECMWF.

Editorial Comments:

P. 17917, Line 14: suggest "One potential reason..." rather than "arguable reason".

P. 17919, Line 5: suggest (Fig. 9, bottom left)

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P. 17919, Line 10: suggest (Fig. 9, bottom right)

P. 17921, Line 26: do you mean "finite interval" instead of "infinite"?

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 17907, 2008.

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Interactive Comment