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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.

N. Lamquin et al.

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Below are all comments and additional comments listed in order of review, answers are given following 'A:'

1 Anonymous Referee 1, 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.

A: more details have been added at the end of section 2.2 on RHi calculations and the uncertainties on RHi have been evaluated using the uncertainties from the specific humidity and the temperature in the AIRS dataset, a new figure is added to the revised version on this purpose.

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2 Anonymous Referee 1, 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.

A: The goal of fig. 1 is to explain in more general terms some issues related to vertical averaging. Details on AIRS vertical resolution are given in section 2.2. We changed Fig. 1 slightly to make the connection with ECMWF a bit more explicit.

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.

A: It is now considered in the revised paper.

Figure 5. I find the discussion of Fig. 5 on P. 17916 to be a bit confusing. It might help 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 do not really find a discussion of the far right column figures in Fig 5 (where you take the RHI difference).

A: for the letters a, b, c... we agree and we made the necessary changes in the text and figure titles. The discussion of Fig 5 has been improved in the course of a complete revision of section 4.1.

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.

A: the lidar is in fact CALIOP but we refer to the data products with CALIPSO as in Lamquin et al.(2008).

P. 17917, Lines 9-12. Can you explain why you see this connection between the

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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?

A: We have now added a couple of sentences that should explain this connection a bit better. The connection is simply: the two conditions “IFS humidity around ice saturation” on the one hand and “clouds detected in CALIOP” on the other are related to very similar distributions of AIRS RHi. So one might conclude that these conditions are nearly equivalent. This means, when CALIOP sees a cloud, IFS has mostly RHi around saturation and vice versa.

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?

A: This is described in section 2.2. The CALIOP footprint is very small compared to the IFS horizontal gridding. Thus as in Lamquin et al.(2008) we can only use CALIOP as an indication of presence, position and size of clouds.

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

A: in fact we added the reference in the list but forgot to insert it in the text. It is corrected.

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?

A: 100 hPa roughly corresponds to 2 km. We added a note in the text.

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

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A: no, it is left-skewed, elongated at the left. The term is confusing since the mass of the distribution is mainly on the right-hand side.

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

A: done.

3 Anonymous Referee 1, editorial Comments

P. 17917, Line 14: suggest "One potential reason..." rather than "arguable reason". P. 17919, Line 5: suggest (Fig. 9, bottom left) P. 17919, Line 10: suggest (Fig. 9, bottom right) P. 17921, Line 26: do you mean "finite interval" instead of "infinite"?

A: all done.

4 Anonymous Referee 2, general comments

The paper presents a comparison of data from the satellite-borne instruments AIRS and CALIOP in order to investigate the performance of the ECMWF model with respect to upper tropospheric humidity. I appreciate the effort taken by the authors, however I do not see really substantial conclusions being reached the way the data are analysed. A more systematic approach (like for example using statistical test to verify/falsify certain assumption) would have yielded more profound results. Also, a more critical discussion of the data that was used is lacking. In particular chapter 4.1 is not very clear. Fig. 5 is a valuable compilation of the data that should be thoroughly discussed. Instead of distinguishing

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a dry and a wet mode, I'd discuss the clear and cloudy cases. In almost all cases AIRS seems to be dry biased compared to ECMWF. This might not be statistically significant except for the most interesting cases in particular the cloudy cases in the higher layers. These differences of the two dataset might be due to the data quality of the measurement. However, I do have the impression that a critical discussion of the ECMWFs new humidity scheme based on these observations is deliberately avoided. Instead, two more figures are presented, that I do not find very helpful in this context (see minor comments for more details). I suggest to rewrite that paragraph focussing on the question whether supersaturation in the model and the AIRS data are significantly correlated or not. Chapter 4.3 should be moved to the front of section 4, because the 'S-shaped' function could play an important role for such an statistical analysis. The correlation with CALIPSO in chapter 4.2 is very interesting. Here, additionaly the resemblance of the 'S-shaped' function in the upper left plot of fig. 9 with the plots in fig. 10 should be discussed. The fact that dry air may contain shallow supersaturated layers, plus the fact that the model is not always predicting the correct humidity fields, might explain the detection of clouds in 'dry' model layers. I suggest publishing the paper after major revision as explained above that I hope do lead also to some more profound conclusions.

A: We see that section 4.1 was not well written, so we have given it a complete revision. The first paragraph of it has been shifted to 2.2. We have added calculations of Pearson's r for RHi_A vs. RHi_E and give now some interpretation of these results. Statistical significance was not a problem due to the large amount of data pairs. We can say that these data sets are statistically significantly correlated but the correlation is low when AIRS has more than 30% effective cloud cover. This points to problems in the AIRS retrieval rather than to problems in the IFS humidity or supersaturation forecast. This does not imply of course, that the IFS is free of problems in this respect. But it is difficult to find them using the AIRS data.

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The dry and wet modes are not so different from the clear and cloudy scenes. In fact the figure (original figure 5) shows the clear sky cases separately from the cloudy cases. However, as evident from the figure, it makes sense to comprise the data into a dry mode (which is mainly marked by the reddish and greenish points along the $y = x$ lines) and a moist mode (marked by the reddish colours near $RHi_E \approx 100$%). In the sense of probability these are maxima in the overall 2D distributions, i.e. modes, and "dry" and "wet" modes are just convenient names that we gave to them.

It is not clear what the reviewer means with "a critical discussion of the ECMWFs new humidity scheme based on these observations is deliberately avoided", since this paper is devoted to a check of the results of the new scheme against observations. Once we notice problems, biases, errors, etc., one can start to look into the scheme and think of improvements. The occasional detection of clouds by CALIPSO when the model has very low humidity should for instance be considered in a case–study fashion. This is, however, not necessarily the job of the authors of the present paper.

The fact that thin ice supersaturation layers can get smoothed away by averaging (e.g. the broad weighting kernels of IR sounders in the water vapour channels) is certainly one of the major sources of inconsistency between AIRS and ECMWF supersaturation. This could be illustrated with the s-function behaviour. However, as the original figure 7 shows, AIRS sometimes shows very large ice supersaturation that are principally possible but are probably erroneous because it is very unlikely that a sufficiently thick layer (> 3 km) has such high supersaturation. Therefore, the s–function issue does not explain all of it, and we prefer to let it where it is.

5 Anonymous Referee 2, minor comments

Abstract: The last two sentences are not very clear. I suggest omitting them and explain some of the more comprehensible conclusions like e.g. that AIRS and

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CALIOP very often detect clouds where the humidity is high.

A: we added some sentences to some of the conclusions regarding the comparisons but kept the last two sentences as we prefer not to move the last part.

p 17912 l. 2: Tobin uses Goff Gratch for calcualting vapor pressure. So using Sonntag's formula is probably not the best choice. However, I certainly will not blame the authors for this inconsitency which is almost inevitable in this context, since everybody uses something else.

A: we have performed different calculations using different formulas (Murphy and Koop, Goff and Gratch, Sonntag) and they all compare within 0.2% relative humidity.

p 17912 l. 29: " accurate" is alway relative. State how accurate, or leave it away.

A: ok, no 'accurate' then.

p 17913 l. 20: Why are old radiosonde data used and not newer one? As far as I know the Meteorological Observatory Lindenberg (use capitals) is still operating and uses better equipment (RS-92) for observing UTH these days.

A: This is simply a matter of convenience. We have these 14 months of corrected radiosonde data ready for use on our computer. They are corrected with the Leiterer algorithm and are stored in the original high resolution (i.e. aprrox. 50 m). The data have been used before, and more details (in particular about their correction) can be found in Spichtinger et al., 2003: Ice supersaturation in the tropopause region over Lindenberg, Germany. Meteorol. Z., 12, 143-156.

p. 17916 l 17 - 27 : not clear! There is in-cloud supersaturation. However in the measurements of the cited literature, RHi always peaks at 100%. The ECMWF-curve in fig. 7 is definately more realstic than the AIRS-curve. Its smooth shape is due to its limited vertical resolution. Please, describe clearer what you were trying to say.

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A: We agree that RHi always peaks at 100%, and that therefore the ECMWF curve is more realistic than the AIRS curve. We reformulate the text in the following way:
When the cloud cover within an IFS grid box approaches 100%, then the relative humidity approaches 100% because in–cloud supersaturation (...) cannot be represented in the current cloud scheme (Tompkins et al., 2007). However, in–cloud supersaturation probably is a transient phenomenon in cloud evolution, hence humidity statistics in clouds always peak at saturation. This is highlighted in figure 8, presenting overall distributions of RHi_E *and* RHi_A *in the pressure layer 300–400 hPa. The presence of the peak in the* RHi_E *curve and the absence of it in the* RHi_A *curve show that the ECMWF distribution is more realistic than the one obtained from AIRS.*

p 17916 l. 28: I guess you are referring to fig. 6

A: Corrected. Please note the change of numbering the figures after addition of figure 3.

p 17918 l. 17: If the cloud is geometrically thick, then the humid layer is also geometrically thick and should be observed. I'd rather guess that the layer is very thin and not represented in the coarser model layers

A: The problem is not that the cloud is not oberved; CALIPSO sees it. The problem is rather that ECMWF either does not predict it or it predicts a very thin cloud embedded in dry layers such that a maximum RHi of 20% results on the coarse levels. The question is thus why ECMWF fails to predict a cloud in certain situations. Without a case-by-case analysis we can only speculate. In the manuscript we have speculated a bit, which is not necessary. Hence we reformulate as follows.
Note that there are a few cases of clouds for which the model predicts low humidity values. ECMWF either does not predict it or it predicts a very thin cloud embedded in dry layers such that a maximum RHi of 20% results on the coarse levels. The question is thus why ECMWF fails to predict a cloud in certain situations. Only case-by-case analysis can help to resolve this problem.

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fig. 5 all three cases (clear, low and high cloud cover) should be plotted the same way.

A: it will in the revised version.

6 R. Forbes, general comments

The paper decribes a worthwhile study utilising different observation sources to assess aspects of ice supersaturation in clear and cloudy situations in relation to the ECMWF forecast model. I have read the reviewers reports and generally support their comments, particularly regarding - a more critical discussion of the data and the need for an improved explanation of AIRS data characteristics/uncertainties to put the results into context

A: See replies to the other reviews.

- a discussion of any impact of using different ice svp formulas

A: done

- a clearer explanation of some of the results/figures in Sect 4.1

A: see reply to referee 2

- a more representative summary of the conclusions in the abstract.

A: done, see reply to referee 2

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7 R. Forbes, minor comments

Figures: Discussion of the figures can be a bit confusing in places. It would be better to label the figures (a), (b) etc. and reference appropriately in the text for better clarity.

A: it will in the revised version.

Fig.3: Why is the lower right hand figure (for april) so much noisier than the equivalent plot for october. It looks like a significantly smaller sample but the fraction of supersaturated grid boxes in the left hand plots are similar ?

A: We cannot really say *why* this happens. We have also looked at September 2005 (Adrian Tompkins made a special run for that month with the new scheme), and at January 2007. In both cases the curves are noisier than for October 2006.

p17910 L6: The horizontal spectral resolution of the ECMWF model for the period used in this paper is T799 (which correspnds to a resolution of 25km), not T511.

A: Corrected.

p17910 L9: Unless I have misunderstood how the data is extracted, T213 corresponds to a resolution of 94km and so it is this that is the effective resolution of the model data (even after being interpolated to a higher resolution 0.5x0.5 degree grid)

A: What we actually did is retrieve the data from the archive in $0.5\,\mathrm{deg} \times 0.5\,\mathrm{deg}$ resolution. We delete the wrong part of the sentence.

p17915 L10: Obvious when you know, but the acronym UTH is not actually defined anywhere. Suggest expanding it.

A: Corrected.

p17916 L28,29: The discussion of Figure 7 is confusing. Is "the IFS cloud cover

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distribution in Fig 7 has larger values than the AIRS effective cloud cover distribution" referring to the section between 60% and 110%, or is it referring to figure 6?

A: We were referring to the then Fig. 6 as referee 2 pointed out. Corrected.

p17921 L6: Sect 2.4 should be Sect 2.3

A: Corrected.

p17921 L7 and figure 10: Could you be more explicit about the depth of layers used in the analysis of the radiosonde data, i.e. what is the resolution of the low res. layers and the high res. data ? The s-function is also a characteristic of the relationship between "cloud fraction" and "total water divided by saturation", averaged over some defined scale (e.g. Wood and Field, 2000, JAS, fig 3.). This is analogous to the relationship seen here between "occurence of supersaturation wrt ice" and "RHi" and is characteristic of an underlying humidity distribution. It is interesting to see it in the vertical for ice supersaturated regions.

A: with the radiosonde data the 'low resolution data' is just an averaging of the data over a pressure layer, because sect. 2.2 now contains more detail on how the average q_s^i is determined we refer to this section.

We believe that the "s-function" can be derived from more general principles, but one definitively needs information on the underlying humidity distribution, their autocorrelation, spectra, etc. When these general principles obtain then the s-function should appear. Hence we are not surprised that it can be found in "cloud fraction" and "total water divided by saturation" as well. We will add the reference.

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