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Atmospheric
Chemistry
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Discussions

# *Interactive comment on* "Probabilistic description of ice-supersaturated layers in low resolution profiles of relative humidity" *by* N. C. Dickson et al.

**N. C. Dickson et al.**

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The authors would like to thank anonymous referee #1 for their comments, the author responses are detailed below.

Comment 1 - The issues of RHI in the context of the thickness of temperature, water vapor, and cloud structures is addressed in detail in several studies previous to this paper. The following paper: Maddy, E. S., and C. D. Barnet (2008), Vertical resolution estimates in version 5 of AIRS operational retrievals, IEEE Trans. Geosci. Remote Sens., 46, 2375 – 2384, doi:10.1109/TGRS.2008.917498 addresses the vertical sensitivity of AIRS temperature and water vapor. Although AIRS data is not explicitly discussed in this paper, it does give some additional context to the problems encountered in assessing vertical structure from remote sensing retrievals and relating them to the structures seen in radiosondes.

A few more papers related to AIRS:

Kahn, B. H., A. Gettelman, E. J. Fetzer, A. Eldering, and C. K. Liang (2009), Cloudy and clear-sky relative humidity in the upper troposphere observed by the A-train, J. Geophys. Res., 114, D00H02, doi:10.1029/2009JD011738.

Kahn, B. H., C. K. Liang, A. Eldering, A. Gettelman, Q. Yue, and K. N. Liou (2008b), Tropical thin cirrus and relative humidity observed by the Atmospheric Infrared Sounder, Atmos. Chem. Phys., 8, 1501 – 1518.

Lamquin, N., C. J. Stubenrauch, and J. Pelon (2008), Upper tropospheric humidity and cirrus geometrical and optical thickness: Relationships inferred from 1 year of collocated AIRS and CALIPSO data, J. Geophys. Res., 113, D00A08, doi:10.1029/2008JD010012.

give some additional context to the vertical resolution problem, and also present climatologies of RHI for thin and thick cirrus as well as clear sky. Some of the lessons learned from these papers could apply to this work or to future applications by the authors if they fold in remote sensing data sets into their research.

Response 1 – The problem of RHi in-cloud pdfs, determined from satellite data, peaking at relative humidities substantially below ice saturation seems to be related to the problem discussed in the present paper. Similar correction procedures might therefore work in both cases. The suggested references (plus some others), along with the following text are included in the introduction.

'A similar problem, to the one discussed in this paper, occurs in relation to the satellite retrieval of RHi within cirrus clouds (RHi_c). While probability density functions (pdfs) of RHi_c, determined from in-situ observations, peak at 100-110% [Ovarlez et al., 2002; Spichtinger et al., 2004], pdfs based on satellite data peak at consistently lower values of around 60-90% [Gierens et al., 2004; Kahn et al., 2008; Kahn et al., 2009; Lamquin et al., 2008]. Therefore, the pdfs based on in-situ observations follow physical expectations (with RHi_c >100%) while the satellite-derived pdfs do not. The most probable reason for this is that cirrus clouds are typically shallower than the satellite instruments vertical resolution, for example, nadir-sounders have a vertical resolution of up to ∼4km for humidity. This is also true for a multi-channel instrument like AIRS [Maddy et al., 2008]. Thus a shallow cloud layer with in-cloud ice saturation or supersaturation, but embedded within a subsaturated air mass (i.e. within the satellite vertical resolution), may result in an overall RHi_c well below 100%. This is suggested in Lamquin et al. (2008) where the pdf peak approaches an RHi of 100% with increasing cloud depth. It may be that the correction procedure derived in the present study would work in a similar way for this related satellite problem, however, this analysis is not included in this paper.'

Section 2

Comment 2 - With regard to the radiosonde observations of temperature and water vapor, what are the effective vertical resolutions? Are they the same for temperature and water vapor? Are the measurements essentially instantaneous or do they have some 'time memory' that smoothes them in the vertical, perhaps differently for temperature than water vapor? If so, how does that affect the vertical structure of RHI from radiosondes? What about problems with horizontal advection as the balloon ascends? Will it smooth over horizontal features increasingly so with higher wind speeds, or are the ISSRs so thin that this does not matter? Some discussion of the radiosonde capabilities and sampling characteristics in the horizontal and vertical is warranted.

Response 2 – The horizontal advection of radiosondes is an inevitable consequence of their use, and there is currently no readily available method to resolve this issue. In this paper the horizontal advection is not accounted for and actually it is not clear how much of an issue this is when understanding vertical profiles of the atmosphere. In fact it is a problem if a vertical profile is considered to be the only and exclusive truth, and consequently a slant (horizontally advected) profile is considered to be untrue. The real problem is defining what a true observation is and whether it is possible to know/find/define the truth. What the current high-resolution radiosonde profiles represent is the closest (current) in-situ representation of the atmospheres vertical profile. The following text will be included in section 2.2.

'An inevitable consequence of using radiosondes to observe the atmosphere is the impact of the horizontal advection on observations, and there is currently no readily available method to resolve this issue. In this paper the horizontal advection is not accounted for and actually it is not clear how much of an issue this is when understanding vertical profiles of the atmosphere. Importantly, the high-resolution radiosonde observations represent the closest (currently) in-situ representation of the atmospheres vertical profile.'

The time-lag associated with radiosonde observations will also be discussed further and the following text will be included in section 2.2.

'The radiosonde time-lag correction (for both RS80 and RS92) is important in this study because it allowed for the recovery of small scale RH structures which may have been smoothed out due to the response rate of the radiosonde instruments [shown in Miloschevich et al., 2004]. The time-lag error itself is a direct result of the humicap sensors non-zero response rate, which is the ability of the humicap to respond to changes in ambient humidity. Actually, the time-lag correction is applied to RH (and not separately to temperature or specific humidity) and is a function of temperature. Although the response rate of the temperature observations are not discussed directly in the published literature [Wang et al., 2002; Miloschevich et al., 2004; Vomel et al., 2006]. Therefore the explicit time-lag differences between observations of temperature and humidity are not considered in this paper. The time-lag correction cannot recover the structure associated with the resolution of the raw observations (∼2 seconds), instead the effective resolution of the observations used in this paper is ∼6 seconds in the troposphere [Wang et al., 2002; Miloschevich et al., 2004; Vomel et al., 2006].'

Comment 3 - p. 2364, line 4: In the case of radiosondes (which is the focus of this work), it probably makes sense to simply average the RHI values. However, what about averaging the temperature and water vapor individually and THEN calculate RHI? Which approach is more robust and justified, or do they yield the same results? Furthermore, going back to the Maddy and Barnet paper, you will see that this is probably not justified for satellite retrieval comparisons because the averaging kernels have a complex structure with height thus implying each point should not be weighted evenly.

Response 3 – The ability to adopt this suggestion (or test it fully) is limited by the radiosonde correction methods used in this study. The necessary corrections are applied to RH (as stated in an earlier response) and therefore averaging humidity and temperature would only be possible if an uncorrected dataset was used. A further reason is directly connected to the averaging technique used in this study, and as averaging and interpolating are similar mathematical procedures this will be explained in terms of interpolation. Specific humidity has an almost exponential profile in the atmosphere with a scale-height of approximately 2km. Accordingly, values of specific humidity cover 3-4 orders of magnitude through the troposphere. A linear interpolation in an exponential function would give incorrect values, and hence interpolation in terms of specific humidity is usually not performed. An alternative would be to take the log of the absolute humidity and linearly interpolate in the log-space. This is similar to interpolating in terms of relative humidity, and therefore there is expect to be little difference (in the shaped curve) between averaging RHi or averaging water vapour and temperature individually.

Comment 4 - p. 2364, line 20: ‘relationship for each’

Response 4 – The sentence: ‘To define a probability relationship each pressure level data point, i.e. (Uk,Sk), was binned by average relative humidity (Uk).’

Will be changed to: ‘Each pressure layer data point (i.e. Uk,Sk) was grouped into average relative humidity bins to determine a probability relationship.’

Comment 5 - p., 2364, line 20 to p. 2365, line 7: There is no such need for complex language for something that is essentially quite simple. Please make it clearer to the reader. For instance, what is ‘the ISS event indicator function’?

Response 5 – The authors propose keeping some of the current text but changing line 5-7 (P2365) to:

‘P (u$\pm$ $\Delta$u/2) now represents the average ISS fraction in any pressure layer within an average relative humidity bins between u=0% and u=140%.’

Comment 6 - p. 2365 onwards: The authors may want to touch on the issue of skewness in the temperature, water vapor and RHI data from the radiosondes, and also the skewness in the derived frequency distributions of ISSRs. Quantifying skewness will explain why, for instance, if 50% of radiosonde points are ice supersaturated (and 50% are not), yet the average in the 50 hPa layer is supersaturated (skewed towards large positive RHI) or subsaturated (skewed towards smaller values of RHI).

Response 6 – If the temperature data is considered, for a certain pressure range, the statistics are not expected to be skewed significantly. Therefore, further investigation of RHi would unlikely help the discussion in the paper, although admittedly pdfs derived from RHi data are not symmetrical and are skewed. This skewed behaviour of RHi is shown in figure 13 of the paper. The s-function itself is asymmetrical around an RHi of 100%, but importantly this is not a consequence of skewness but instead is related to the varying spread in the data (the ïĄşu) depending on u. A comment regarding this will be made at the end of section 4.

Section 5

Comment 7 - Do the authors have some physical insight as to why there is some interannual variability in the frequency of ISSRs with respect to RHI in the thicker layers? This behaviour seems consistent between most of the stations (Fig. 6, 2006 has a less sharp S curve). The authors note that the radiosonde instruments used changed at this time, but could there be a physical reason? How does a change in the ‘response time’ affect the vertical resolution of the radiosonde observation? Why don’t the authors use data from 2007 and onwards, which would provide additional insight on determining the cause of the change from either atmospheric processes or a possible instrument change?

Response 7 – The most probable reason for the difference between the 2006 dataset and other datasets (2002-2005) is the change in radiosonde instrument (from RS80 to RS92). The effective resolution (∼6 seconds) of both RS80 and RS92 observed profiles are the same (they both use the same time-lag correction algorithms) [Vomel et al., 2006]. However, the RS92 offers an improved calibration accuracy and faster response time when compared to the RS80 [Paukkunen, 1995]. The faster response of the RS92 to identify the change between sub-saturated and supersaturated air masses (compared to the RS80) is significant. This means that the vertical spatial extent of an ISS region, observed with an RS92 radiosonde, will be smaller than if an RS80 radiosonde was used. To explore the impact of the RS92 instrument further, an extension of this study would be to use 2007, 2008 and 2009 datasets (which are now fully available). The authors will state this as a recommendation (along with further investigations using tropical datasets) in the conclusions of this paper.

Comment 8 - Figure 9 is confusing because the same curves in the left column are repeated on the right. The only difference the reviewer could see is the addition of the two 100 hPa curves. Why not simply plot the right column?

Response 8 – Agreed. The graphs in the left hand column of Fig 9 will be removed from the paper.

Comment 9 - p. 2368, lines 20-21: It is true that the tropopause is colder and drier, but that does not mean RHI is smaller. See, for instance, Kahn et al. (2009), or search for some UARS/Aura MLS papers on this.

Response 9 – Indeed, RHi can be large near (and above) the tropopause, but this is the

exception rather than the rule. For example, radiosonde data [e.g. Spichtinger et al., 2003] shows that ice supersaturation occurs rarely in the lowermost stratosphere. The RHi pdfs in the lower stratosphere are exponential from 20% up to high supersaturation, as shown by MOZAIC data [Gierens et al., 1999] and MLS data [Spichtinger et al., 2002]. This implies that the lower stratosphere is statistically much drier (in terms of RHi) than the upper troposphere.

Comment 10 - p. 2371, lines 28-29: Have the authors used any of the high quality radiosonde data from the Atmospheric Radiation Measurement program (ARM) sites in the tropical western Pacific? Could be a nice contrast to the single tropical station shown here.

Response 10 – The authors have not used any of the tropical western pacific ARM radiosonde profiles. Analysis of these data would make an excellent comparison to the single tropical station in this paper. The authors suggest that this is strongly stated in the conclusions as recommended future work.

Comment 11 - p. 2373, lines 12-14: Here the authors address skewness of RHI, could expand on with the other figures of ISSR frequency as a function of RHI.

Response 11 – The skewness is mentioned here only for a complete description of the pdfs. The mathematical function (the error function) constructed later in the paper does not use this skewness. In fact even without considering this skewness the mathematical function represents the s-function to within acceptable limits. This shows that skewness can be ignored in the mathematical function. Actually, incorporation of this skewed RHi behaviour would lead to a more complicated mathematical formulation, because the gaussian pdf and its known cdf, the error function, could no longer be used. There appeared to be little gain in making the model much more complicated, particularly given the inevitable statistical noise which would be in every application.

Comment 12 - Figure 4: Why not make a 2-d contour plot of the frequency? It would be much easier to see which combinations of RHI and ISSR frequency are the most/least populated.

Response 12 – This will enhance the understanding of the relationship between RHi and ice supersaturation. This new plot will be included as figure 4b (with the current figure 4 as figure 4a). (see figure 1 of this response document)

The caption for figure 4 now reads:

'Relationship between the average RHi (Uk in %) and ISS fraction (Sk) within 50hPa layers (400–150hPa). These data are derived from five years of UK Met Office high resolution radiosonde launches from Camborne, Castor Bay, Herstmonceux, Albemarle, Lerwick and Watnall launch stations (January 2002–December 2006). Panel (a) shows each individual 50hPa pressure layer from the full dataset. Panel (b) shows a contour plot of the probability distribution of the full dataset, for clarity (in the contour plot only) data from pressure layers of an ISS fraction=0 (zero ice supersaturation) and an ISS fraction=1 (fully ice supersaturated) were removed.'

Comment 13 - Fig. 14: 250-300 and 200-250 colors are too similar, can't tell apart.

Response 13 – The colours in figure 14 will be altered for clarity

Comment 14 - Could the authors elaborate a bit more on how this work could be used to parameterize contrail formation in climate models?

Response 14 – To elaborate more on using this work to parameterize contrail formation, the following will be appended to the conclusion section.

'The coarse horizontal and vertical resolution of climate models mean a single RHi value describes a grid box of, for example, 200km ïĆť 200km ïĆť 1km. Subgrid-scale variations of RHi are generally accounted for by cloud formation parameterisations which allow clouds to form at RHi values that are lower than cloud physics postulates. This is the equivalent to assuming that in one part of the grid box the RHi is such that a cloud can form while the grid-box mean RHi is still too low. The same logic holds true for contrail formation and contrail persistence, with the understanding that contrails will

only persist when the atmosphere is ice supersaturated (RHi>100%). Therefore, in a climate model grid box (given an average RHi), the s-function model could be used to interpret the ice supersaturation content within a grid box depth. For example, applying the s-function to a grid box with an RHi of 80% would mean that 20% of the boxes depth is ice supersaturated, which describes the potential for contrail persistence.'

References (to be included in the paper)

Gierens, K., U. Schumann, M. Helten, H.G.J. Smit, A. Marenco, 1999: A distribution law for relative humidity in the upper troposphere and lower stratosphere derived from three years of MOZAIC measurements. Ann. Geophys. 17, 1218-1226.

Spichtinger, P., K. Gierens, W. Read, 2002: The statistical distribution law of relative humidity in the global tropopause region. Meteorol. Z. 11, 83-88.

Spichtinger, P., K. Gierens, U. Leiterer, H. Dier, 2003: Ice supersaturation in the tropopause region over Lindenberg, Germany. Meteorol. Z., 12, 143-156.

Maddy, E. S., and C. D. Barnet (2008), Vertical resolution estimates in version 5 of AIRS operational retrievals, IEEE Trans. Geosci. Remote Sens., 46, 2375 – 2384, doi:10.1109/TGRS.2008.917498

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Lamquin, N., C. J. Stubenrauch, and J. Pelon (2008), Upper tropospheric humidity and cirrus geometrical and optical thickness: Relationships inferred from 1 year of collocated AIRS and CALIPSO data, J. Geophys. Res., 113, D00A08, doi:10.1029/2008JD010012.

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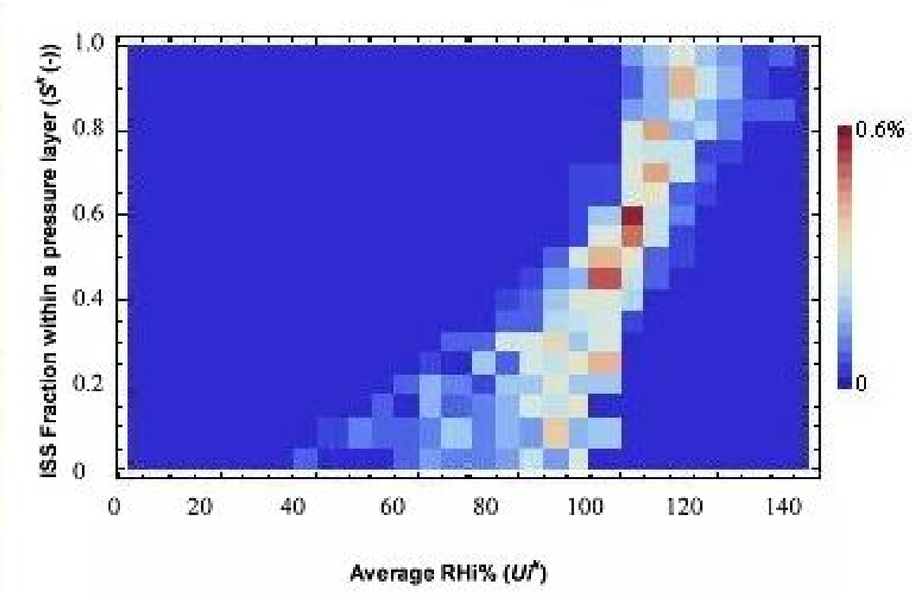

**Fig. 1.** Figure 4b (see response text for the full figure 4 caption)