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Atmospheric Measurement Techniques Discussions

Interactive comment on "Matching radiative transfer models and radiosonde data from the EPS/MetOp Sodankylä campaign to IASI measurements" *by* X. Calbet et al.

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We would like to thank the referee for the provided comments. Relevant comments from the referee are reproduced in bold letters for clarification.

Abstract is poorly written. It does not say anything about the results. It has to be rewritten.

Please note there has been two versions of the manuscript. The abstract has been modified in this second version. We hope it is clearer now.

Under polar conditions, can one discard contribution from low clouds and surface completely? Any sensitivity studies have been done to check this?

Different IASI channels get radiation from different parts of the atmosphere. Highly absorbing channels get their radiation mostly from regions high in the troposphere and stratosphere. These are the ones selected for this study. Therefore, these channels are not affected by lower layers of the atmosphere, if in particular, they contain clouds or other surface emissivity effects.

In any case, if the radiation at these wavenumbers were truly affected by clouds or surface effects, it would be impossible to match the clear sky calculated radiances with the measured ones. An effect of this can actually be seen in Fig. 6 to 8 between wavenumbers 1570 and 1615 cm-1. These channels are affected by low level clouds observed with AVHRR and therefore do not match well.

Figures 6-8 do not contain simulations for RS92 flown together with CFH. Why is it not shown?

The CFH measurements are taken as a reference. They are taken as a very accurate knowledge of the atmosphere at the places were it is measuring. Therefore, althoug it would be interesting to show the accompanying RS92 sondes as the referee suggests, there is no real need to show them. Also these figures are quite busy already and adding a new radiance calculation would increase the confussion.

But please note, that RS92 measurements flown together with CFH are used to calibrate the "in situ" bias correction.

Higher variability in OBS-CALC for smaller wave numbers and vice versa: Any reasons?

Please note tha Fig 14 shows a higher standard deviation for higher wavenumber, which seems to suggest the opposite of what the referee mentions.

Please note also that there is a "window" between 1570 and 1615 cm-1 which is af-

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fected by low level clouds. We do not expect any proper match between calculated and measured radiances here, that is why this region is removed when plotting the histograms Fg. 9-13.

Did you take the closest pixel to the station? To see how inhomogeneous the scene for each wave number you can calculate variance of pixels within a circle around station with certain radius and it can be plotted also in Figures 6-8. The radius of the circle can be determined from sonde track. Use the maximum distance it has drifted from the station.

Yes, the IASI pixel is the one closest to the station. This is not clear in the text and will be clarified. We wish to thank the referee for pointing that out.

The technique the referee mentions here, that of getting error bars using spatial inhomogeniety is one we have used for other studies where the matches are usually not that good (cloud top pressure determination or cloud fraction) and error bars are a must. In this case, the agreement between calculated and measured radiances is fair enough not to pursue anything further at this stage of the study. Please note that IASI pixels are separated very much appart, about 25 km. This fact was unfortunately not mentioned in the paper, but thanks to the comments from both referees it will be included in the final version. In any case, this separation of IASI pixels makes the radiances from one pixel relatively different from a neiboughring one, at least to the extent of the errors appreciated here (one sigma IASI instrument noise), which would possibly make these spatial inhomegeneities too large to be useful proxies of radiance errors. In any case, it is a good point worth considering for future studies.

What are the legends in Figure 2 (e.g., 2007.61.5)?

This was a mistake in the figures which has been corrected in this second version. They were meant to be the dates.

Time interpolation does not seem to have much influence. Bias for corrected

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RS92 -5 min is close to the interpolated profile.

It does not have much difference in the particular sondes shown in Figs. 6-8, but when you want to fit all four calculated radiances measured at four different days then it is absolutely neccessary. This point will be clarified in later versions of the text. We wish to thank the referee for pointing that out.

CFH and uncorrected RS92 launched 5 minutes before show similar magnitude of bias.

But of different signs! Again, different days show different behaviour. Not all days have been plotted in the paper.

For wave numbers which are sensitive to lower stratosphere, CFH should show good agreement. Make a plot for OBS-CALC for only such frequencies.

The channels selected for this study are sensitive exactly at these levels for water vapour.

Figures 9-12 can be combined to one figure. In present form it is hard to understand the differences, e.g., what is the difference between two versions of LBLRTM? And this is only a qualitative analysis. More quantitative analysis should be done. What is the reason for using 3 RT models? Why a fast RT model better than a line-by-line model? There is no systematic approach here. Why should a correction method depends on the RT model to get a better fit?

We have chosen to plot the histograms separately for clarity reasons. Otherwise the figure would be too busy. It is already hard enough to distinguish between the two histograms and the Gaussian curve in the figures because of their almost perfect match.

The reason for using 3 RT models is to verify as much as possible the differences or similarities between different RT models. Perhaps this is not sufficiently clear in the text and will be clarified. We wish to thank the referee for this.

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A fast RT model is used because this kind of models are the ones used in operational evironments (EUMETSAT) for their speed. So they are important to us. This point is not sufficiently clear in the text and will be clarified in future versions. We wish to thank the referee for pointing that out.

Different RS92 bias corrections have been tried to see which one fits the data better. We can see from the paper that there is no way to distinguish which of the RT models matches better, with the present instrument accuracy.

We kindly do not agree with the referee here. This is a quantitavie study, where histograms have been plotted and an bias and standard deviation of the measurements has been performed (Fig. 14). We could agree that the results are statistical in nature, but they certainly are quantitative.

Why does 2007-07-25 (Fig. 14) profile show larger bias compared to others?

Perhaps becuase this day has more variability or the sonde has drifted further away. We did not look into the fine details of the discrepancies here. Note that the results from 2007-07-13 are also not as good as the remaining two. We did verify that to get an agreement of all the sondes it was mandatory to bias correct and time interpolate.

Please note that an agreement within one sigma IASI instrument noise is a very fine grained agreement. This can be seen in Fig. 12, were an addition of 2% absolute relative humidity makes the observations and calculations match. That means we are seeing differences in two percent relative humidities along the whole profile. This is very fine grained indeed.

We wish to thank the referee for his/her comments.

Interactive comment on Atmos. Meas. Tech. Discuss., 3, 4497, 2010.