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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Combining visible and infrared radiometry and lidar data to test ice clouds optical properties" *by* A. Bozzo et al.

Anonymous Referee #1

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GENERAL COMMENTS:

In this manuscript spectral (upwelling) radiance measurements from mostly three airborne instruments (MODIS Airborne Simulator, MAS, Scanning High-resolution Interferometer Sounder, S-HIS, and Cloud Physics Lidar, CPL) have been analyzed and compared with the output of sophisticated radiative transfer simulations. Clear and cloudy cases have been selected from a field study (airborne data) and from satellite overflights (MODIS). The simulation results agree well with the measurements for the clear cases; for the cloudy examples serious deviations are ascertained for the airborne solar and the near-infrared (NIR) spectra. Here we have a major problem of the paper: No convincing reason for the model-measurement discrepancies is given by the authors. Even more puzzling is the fact that the satellite data from the selected

MODIS overflight under cloudy conditions are in good agreement with simulations. As long as no solid explanation (other than speculation on the ice cloud optical model) for the measurement-model discrepancy is given, the paper is of limited value only.

A second major issue is the lack of in situ cloud measurements and related uncertainties. This cannot be changed anymore, the measurement campaign is over. However, I suggest the authors systematically re-check the sensitivity of the simulated radiances with regard to uncertainties in the crystal size distribution.

Despite of these two major issues I like the paper because it is very open in admitting the problems. It is well written, although sometimes it is not easy to follow for readers which are not familiar with this subject. In general it is an interesting paper which I recommend for publication after considering the two general and specific issues.

SPECIFIC COMMENTS:

- Title: I think this is not quite appropriate. How "ice cloud optical properties" can be "tested"? The paper is more on combined measurements and respective simulations of reflected radiances from the solar to the IR spectral range.

- Abstract: Please explain the acronym THORPEX (even though this is well known; still, each acronym deserves explanation). The CPL-derived optical thickness agrees with that obtained from S-HIS, that might be a random result. I actually do not see a reason why these data should always agree. The optical thickness is a function of wavelength, even for clouds. Also, even if both data sets agree in this specific case this does not necessarily justify using the IR optical thickness for the solar and NIR radiance simulations, in my point of view. Maybe this causes all (or at least part of) the trouble in the comparison?

1. Introduction

- This is quite well written, a concise introduction into the subject of the paper.

2. Description ...

- What means the acronym P-TOST, did I miss something here?

- Maybe an additional table would help to summarize the specifications of the three major instruments: MAS, S-HIS, CPL

- The meteorological situation is introduced in just a couple of words. That could/should be done in more detail.

- The problems with the wrong geographic data described in 2.1 sound strange;, how can such expensive radiance measurements be endangered by such basic problems in supplementary data? Nevertheless, the authors obviously did a good job in order to circumvent these problems. I would agree if the authors omit mentioning these problems, they are not the key to this paper.

- I don't understand the claim that LbLRTM was "used to generate layer monochromatic optical depths (OD)". I thought the model uses OD as input and simulates radiances? Please elaborate ...

- I know that Figures of the type of Fig. 1 are the common quicklook output, still I don't feel this is appropriate for a paper. Maybe you redraw ...

- Page 8, third para: What do you call "particles"? Does it include ice crystals and water droplets or just the ordinary aerosol particles?

- What means "un-apodised spectra", I have never heard of that term.

- On page 9 and 10 the particle size distribution is introduced. No convincing justification is given for the choice of a gamma type of distribution (well known to work for warm clouds with liquid water droplets). Later it is mentioned that the parameter μ is assumed to vanish, N0 and lambda are not specified, at least I did not find something in the paper on these parameters. This is a major weakness of the paper and I suggest the authors include a section on systematic sensitivity tests of the radiance simulations with regard of size distribution assumptions.

- It is mentioned that particular attention has been devoted to test the effects of using a very limited number of Legendre coefficients for the highly asymmetric phase function of ice crystals. I would like to encourage the authors to show more results of these efforts in the paper. This is always a major difficulty in radiance calculations for cirrus and it would deserve some journal space here.

3. ... clear cases ...

- Fig. 2 is not really useful for the clear case, it could easily be omitted.

- Is there any independent source of information for vertically integrated aerosol optical depth (e.g., from MODIS). Even just a small value of 0.07 in the clear sky case might be important. I agree it is not an issue in the cloudy cases. Also, why the CPL cannot deliver data below 1000-500 m altitudes?

- How the pigment concentration was included in the radiative transfer simulations?

- Error bars in Fig. 3 could generally be omitted (the hint in the text is okay). Error bars get noticeable in later figures. Talking of error bars: What you show are variabilities, are there any estimates of real error bars available?

- page 16, third para: Could you give some more explanation what the "6S model" includes (you just give the reference Vermote)?

- Fig 5: The red curve is almost not discernable.

- Fig. 6 should be supplemented by a graph showing the percentage differences.

4. ... cloudy cases ...

- Now Fig 7 makes perfect sense, compared to Fig. 2

- Fig. 9 might be implemented into Fig. 8 which should be more discussed. How do I see that the signal is saturated, because it gets red?

- Can you explain how you have selected the three specific cloud areas. They are not

equally long and the cloud period between the green bars contains a spike. Can you somehow reason your choices?

- Fig. 10: Why don't you treat the MAS channel 1848 cm-1 not the same as in Fig. 3, simply show it and call it bad.

- Figs. 11 and 12 bring up a major problem. The PSD as source of error was ruled out, though I am not convinced of that. Did the authors look for consistency between the PSD and the OD? If that already fails, then the PSD causes trouble already.

- To me it does not really help to include the MODIS case study. It somehow leaves the reader even more concerned (not to say suspicious) because to me it does not give solid evidence that the ice crystal optical model causes the discrepancy. This MODIS case is so different from the previous cases, I don't know how this may fit into the major problem of the manuscript, the missing match in the model-measurement comparison for the solar and NIR radiances of the previous cloud cases. I am not as native speaker, but I would call this MODIS case a little "far-fetched", if you know what I mean.

- I did not understand Fig. 16 where you have plotted radiance differences, regarding the text. Can you elaborate more clearly to which difference you are referring here? In Figs: 16, 17, 18(lower panels) values on the y-axis are given in percent, that should be done in a consistent way in similar figures (e.g., Figs. 4 and 14 upper two panels).

5. Conclusions ...

- The discussion of phase function problems in explaining the radiance discrepancies is not convincing, rather speculative.

- Wouldn't it help to use the MODIS optical thickness in the cloud cases of PTH? It looks to me that the OT from the IR and the CPL measurements causes all the problems? Could you please check this option?

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 7215, 2010.