Part 0. All reviewers have expressed the following concerns:
1) the sensitivity of simulated radiance to uncertainties in the assumed size distributions
2) the sensitivity of simulated radiance to the hypothesis of an homogeneous distribution of ice mass in the whole cloud dept.

These points will be answered in what follows and reference to this section will be made under each reviewer's comment.

0.1) ***Sensitivity of simulated radiance to uncertainties in the assumed size distributions***

We have applied RT-RET to the same three sectors considered in the paper and retrieved the infrared cloud optical thickness (OT, the integrated cloud optical depth), the effective diameter (De, microns; see below at point 2.6 for its definition) and the ice amount (IWC, gr/m^3) assuming two new sets of size distributions (26 Gamma type PSDs for each set). The mixture of habits is the same for all the 3 sets of PSDs (the two new set and the old one). The Gamma PSD is defined as:

$$n(D) = N_0 D^{\mu} e^{-\lambda D}$$

where D is the particle's maximum dimension, λ is the slope, μ the dispersion (or spread), and N_0 is the intercept. The latter determines the total number of particles in the PSD, constraining the IWC (and thus is a parameter derived from the retrieved OT and De). Changing N_0 merely shifts the PSD up or down (in a *D-n(D)* plane). For this reason once the particles' habit type, PSD slope and dispersion are determined, the spectral behavior of the computed optical properties (i.e. K_e, ω and g) does not change. The parameters used to define the PSDs are given in Table 0.1-1. The main parameter defining each set of PSDs is μ (the dispersion). For each μ (i.e. each PSD set) the slope is varied so that 26 PSDs are defined by their effective dimension. Typical values of the dispersion μ are between -2 and 16, while typical slopes, λ, range from 0.1 to 10^{-4} cm^{-1} (with D expressed in cm). These values are derived from data discussed in Baum et al., 2005 and Heymsfield et al., 2004, where measured PSDs of ice clouds from 6 field experiments are fitted to theoretical modified gamma type PSDs.

B. A. Baum, A. J. Heymsfield, P. Yang, and S. Bedka, "Bulk scattering properties for the remote sensing of ice clouds. Part I: Microphysical data and models," J. Appl. Meteor., vol. 44, pp. 1885-1895, 2005.

A. J. Heymsfield, A. Bansemer, C. Schmitt, C. Twohy, and M. R. Poellot, "Effective ice particle densities derived from aircraft data." J. Atmos. Sci., vol. 61, pp. 982-1003, 2004

Table 0.1-1

name	μ (dispersion)	λ (slope) [cm^{-1}]	Notes on the shape of the PSD
V0	0.0	26 different values	Exponential; shown in manuscript
V1	7.0	26 different values	under-exponential
V2	-0.1	26 different values	over-exponential

Table 0.1-2 reports the retrieved values (IWC, De and RT-RET OT(IR)) for the 9 cases. The last row in Table 0.1-2 is the OT at 532 nm, the lidar wavelength, computed from RT-RET OT(IR) using the same size distribution and particle mixture.

Table 0.1-2

	Sector 1(red)			**Sector 2(green)**			**Sector 3(blue)**		
	V0	**V1**	**V2**	**V0**	**V1**	**V2**	**V0**	**V1**	**V2**
IWC(mg/m3)	8.9	7.3	8.9	11.7	10.1	11.4	45.4	40.8	48.4
De	68	56	52	64	56	48	80	72	64
RT-RET OT(IR)	0.53	0.58	0.55	0.95	0.99	0.95	2.80	2.77	2.72
RT-RET OT(SW)	0.57	0.62	0.59	1.02	1.07	1.03	2.99	2.94	2.90

The three retrieved PSDs (shown in Figure 0.1-1 for the PSDs used in Sector 3) were used to simulate the spectral radiance at high resolution in each of the three sectors, and the S-HIS and MAS convolved radiances. The difference among the results for cases V1 and V2 are plotted in Figures 0.1-2 to 0.1-4 respectively for Sectors 1 to 3. Fractional radiance difference (L(Vx)-L(V0))/L(V0) is plotted for the SW and NIR ranges, while brightness temperature difference is plotted for the IR range.

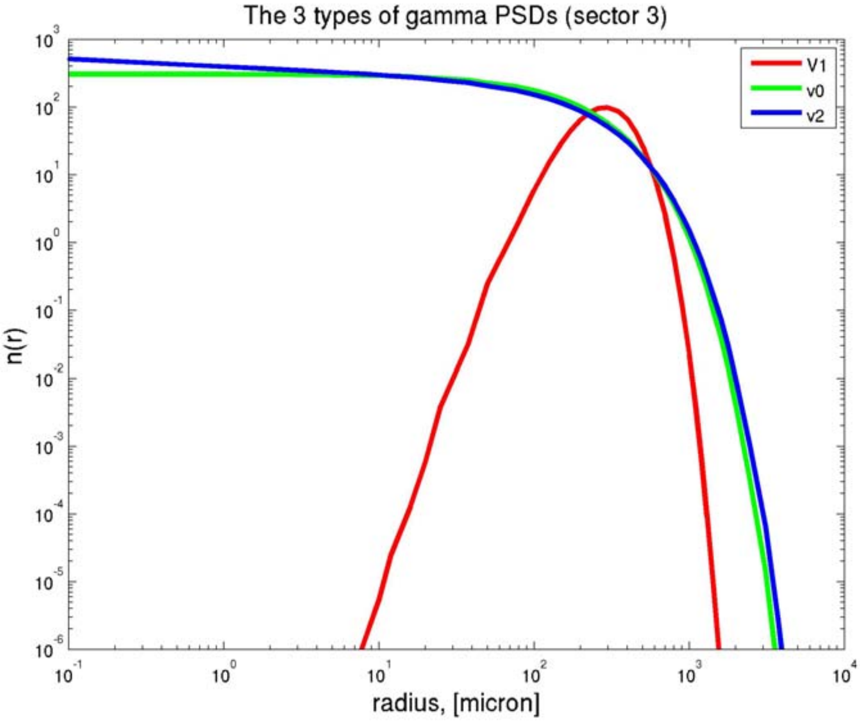

Figure 0.1-1: The three PSDs retrieved by RT-RET for Sector 3. The corresponding De are reported in Table 0.1-2. n(r) is in units of number of particles per microns per volume (m^3)

The maximum difference in the SW range is for case V1 in Sector 3 and is of about 0.02 (or 2%). In the NIR range maximum difference is around 2 microns and is less than 10%. In the IR range the absolute differences are below 0.3 K except at 2650 cm-1 in Sector 3 (less than 0.6).

The three PSD used are quite different; yet the difference in simulated radiances for the three PSDs are much smaller than the difference between measured and simulated radiances described in the manuscript.

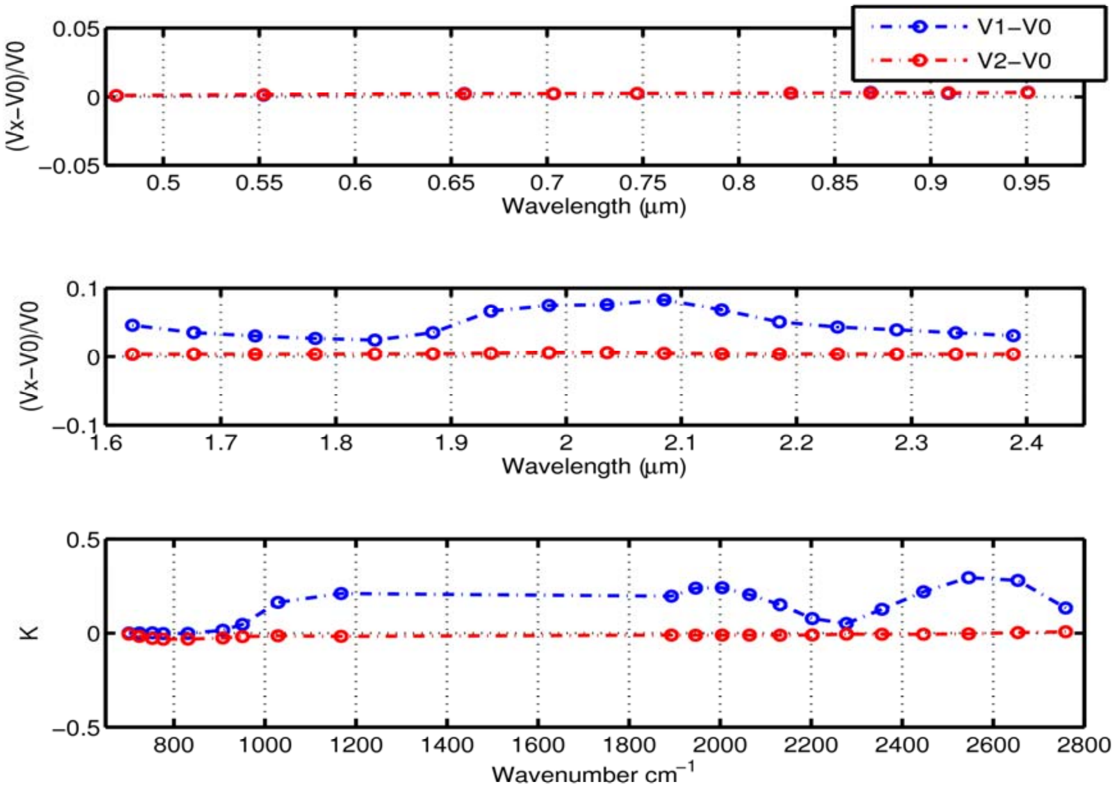

Fig. 0.1-2 – Top and middle panels: fractional difference for Sector 1; lower panel Brightness temperature difference.

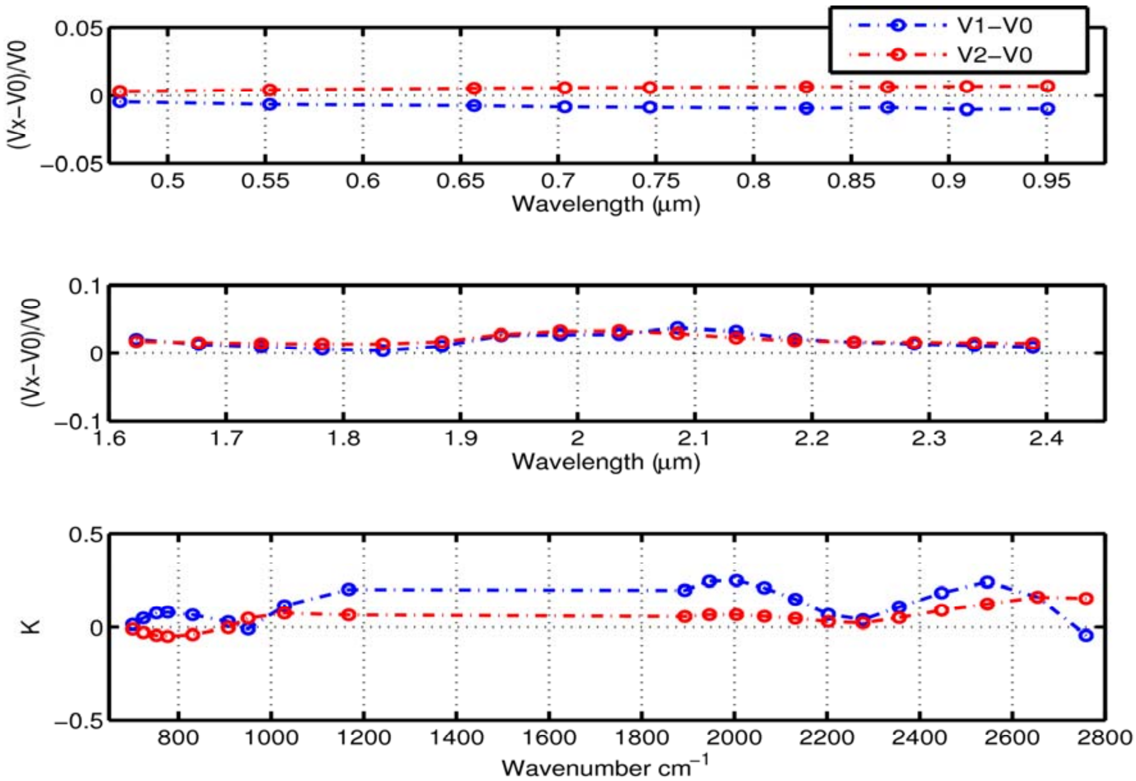

Fig. 0.1-3 - Top and middle panels: fractional difference for Sector 2; lower panel Brightness temperature difference.

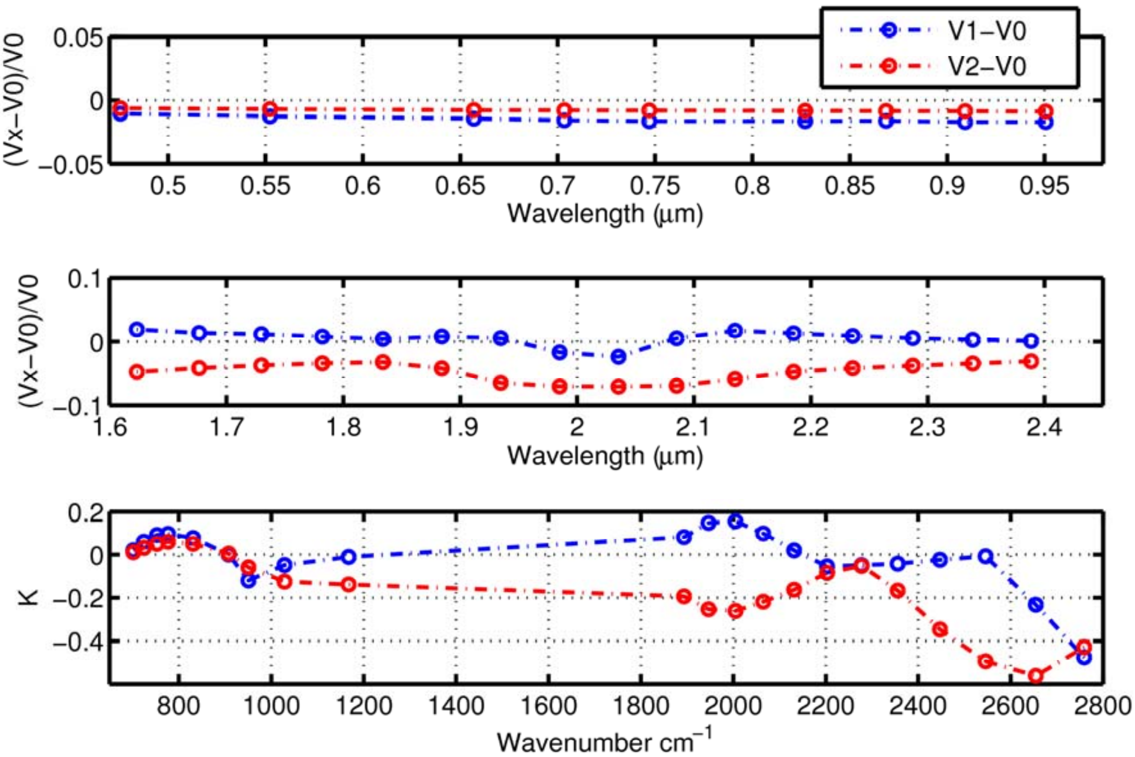

Fig. 0.1-4 - Top and middle panels: fractional difference for Sector 3; lower panel Brightness temperature difference.

0.2. On the effect of non-homogeneity of the cloud layer.

The results for the homogeneous case were shown in the manuscript in order to be consistent with RT-RET that retrieves cloud parameters assuming that the cloud is homogeneous. To answer the comments by all referees we have used the CPL derived cloud extinction profile to modulate the amount of ice mass within the cloud layer in each sector. We have used same effective diameter (for the exponential PSD, i.e. case V0 above) retrieved by RT-RET assuming an homogeneous cloud layer. We have not attempted to modify the PSD, or the De or the type of ice crystal mixture in each layer, as we have no data to warrant any choice of this type.
The retrieved cloud properties are then used to compute the optical properties and to calculate the total ice amount so that the OT at 532 nm matches the CPL one (given in Table 1 of the manuscript - CPL OT and in Table 0.1-1 of this document).

The average CPL extinction profiles for the three chosen sectors are shown in Fig 0.2-1. The extinction profiles are interpolated at the model levels (computations in sectors 1/2/3 are done with 11/14/14 cloud layers, of thickness 0.2 km, out of 81 layers to describe the atmosphere) and the IWC profile is computed so that it is proportional to the average extinction profile. The final IWC profiles are shown in Fig 0.2-2.

We already know from first principles that radiance in the infrared is very dependent on the vertical distribution of mass since the latter changes the source function profile which, being mostly due to emission, is a function also of temperature. Our interest is however centred in the shortwave and near infrared.

Fractional differences (Inhom-Hom)/Hom between the inhomogeneous and the homogeneous cases for the three sectors are shown for the shortwave and near infrared MAS channels in Fig. 0.2-3. Our computation gives largest fractional differences in sector 1 in the whole spectral range. The fractional differences increase with increasing wavelength in the shortwave and stay nearly constant in the near infrared, except between 1.8 and 1.9 microns where molecular absorption is relevant (as can be seen in Fig. 0.2-4). Note however that, for the present case, the inhomogeneity reduces the scattered radiance to the sensor with respect to the homogeneous assumption.

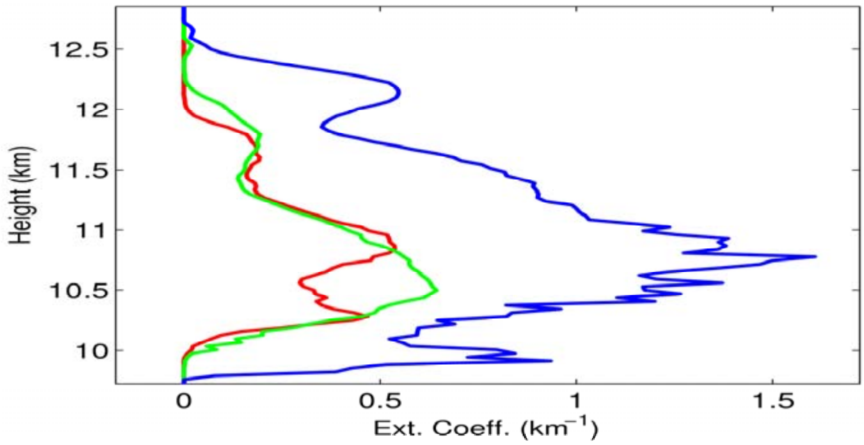

Fig. 0.2-1 – Average extinction profile for Sector 1 (red), 2 (green) and 3 (blue) derived from CPL.

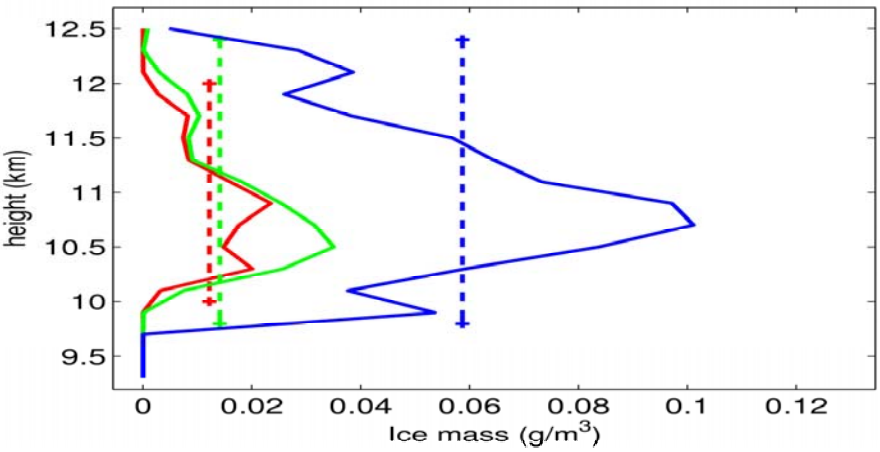

Fig. 0.2-2 – Average IWC profile, for the LBLMS layering, for Sector 1 (red), 2 (green) and 3 (blue) derived as explained in the text. The vertical dashed lines indicate the profiles adopted for the "homogeneous" computations.

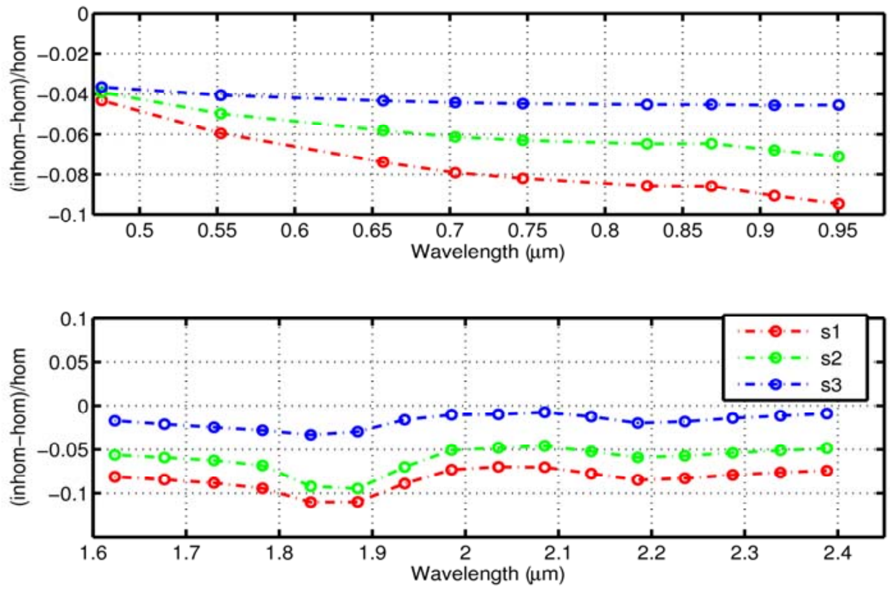

Fig. 0.2-3 – Fractional difference (Inhom-Hom)/Hom between the inhomogeneous and the homogeneous cases for the three sectors, in the shortwave (top panel) and near infrared (bottom panel) MAS channels.

We did make many other simulations, before submitting the manuscript and also while waiting for the reviewers' comments, also using very extreme distribution of ice mass, such as putting 90% of the total ice mass in the upper half (case INHa) or in the lower half (case INHb) of the cloud deck. An example of these results is shown in Fig.0.2-5. The plot shows that the fractional difference between the two extreme cases (INHa- INHb)/HOM is smaller than 1% in spectral regions where atmospheric molecules have moderate absorption, and below 10% in most absorbing regions except the strongest.

We can quite safely conclude that the difference in simulated radiance for the homogeneous and the inhomogeneous cases are, in any case, much smaller than the difference between measured and simulated radiances described in the manuscript.

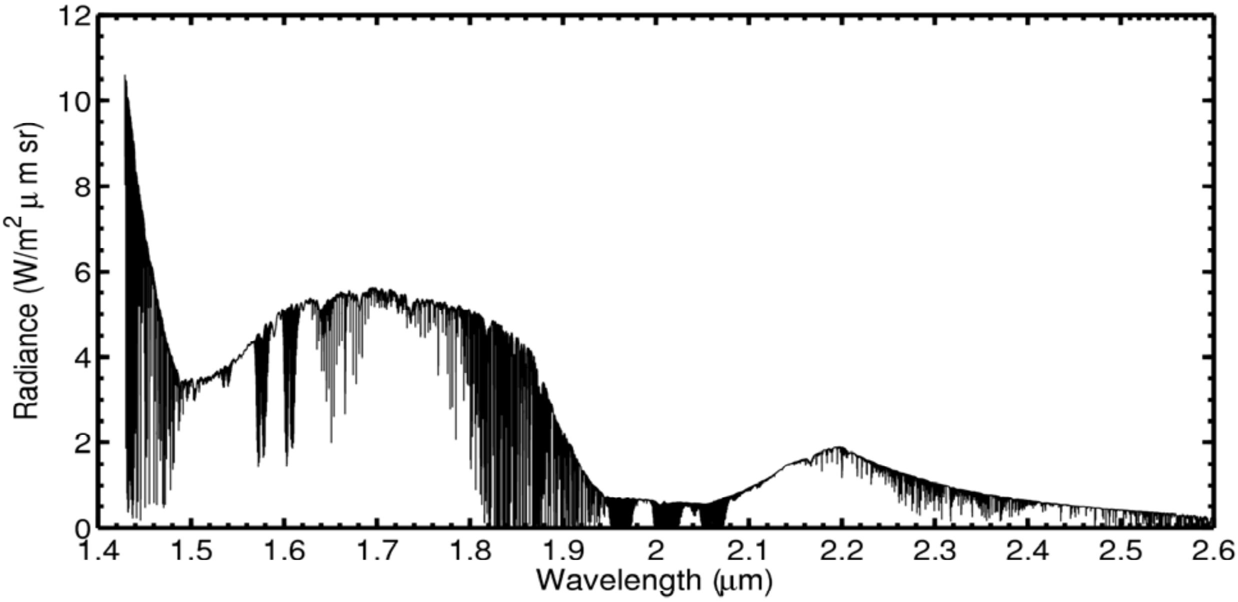

Fig. 0.2-4 – Simulated spectra for sector 3 case V0 for the near infrared spectral range.

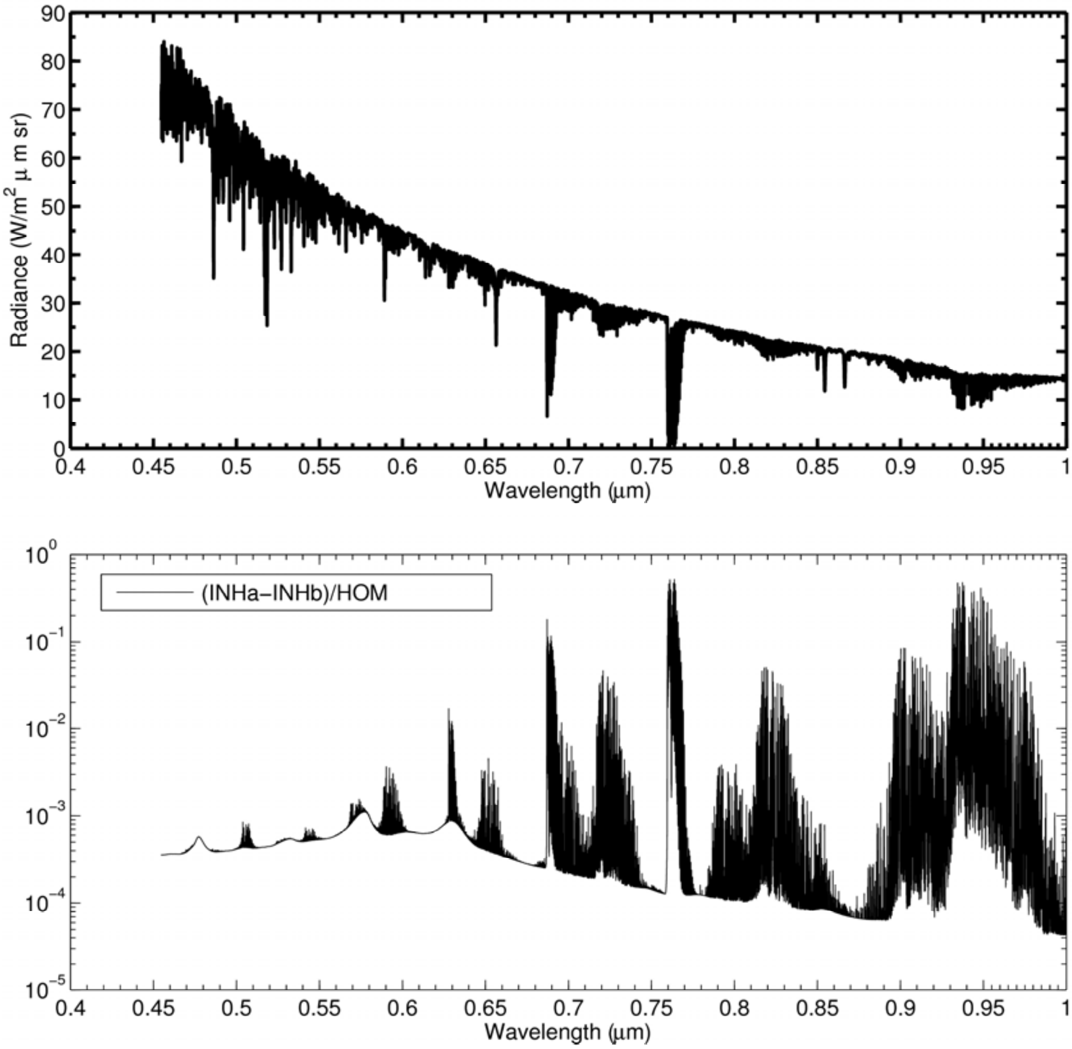

Fig. 0.2-5 – Radiance spectrum (sector 2, PSD V0) in top panel and fractional difference between case INHa and INHb (bottom panel: note the logarithmic scale along the ordinate axis).

***I**nteractive comment on "Combining visible and infrared radiometry and lidar data to test ice clouds optical properties" by A. Bozzo et al.*
Anonymous Referee #1 Received and published: 30 March 2010

Question numbering is added to improve cross-reference with replies to all reviewers.

GENERAL COMMENTS:
In this manuscript spectral (upwelling) radiance measurements from mostly three air-borne instruments (MODIS Airborne Simulator, MAS, Scanning High-resolution Inter-ferometer 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 air-borne 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.

The above general comment is answered at reply 1.30.

1.1) 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.

The basic elements of our explanation are the following:

a. LBLMS simulations in cloudy conditions strongly underestimate MAS radiances in the SW and NIR when cloud parameters are derived from infrared retrievals;
b. LBLMS simulations are in good agreement with MODIS short-wave measurements when cloud parameters are derived from same MODIS shortwave channels;
c. the OTs at 532 nm derived from the RT-RET retrieval from S-HIS IR data agree with OT measured by CPL at 532 nm.

The essence of any retrieval algorithm is to derive cloud parameters that, when used in the same forward model used for the retrieval, reproduce (to within well defined, and usually small, errors) the measured radiances. Point b) shows that our cloud modeling (PSD, databases of optical properties – scattering coefficients and phase function) used with LBLMS is sufficiently close to the cloud modeling used by the MODIS team. This closeness does not imply that the optical properties (scattering coefficients and phase function) are correct, since same properties are used in both the retrieval and the forward model.

The radiance emerging from the cloud layer is proportional, in conditions of single scattering, to the product of direct solar radiance reaching the cloud layer, of the scattering coefficient and of the phase function. Therefore the results outlined above as points a) and b) would imply (in condition of single scattering) that the discrepancy is to be ascribed to the phase function adopted, because of the point c) above.
In the actual conditions (multiple scattering) the above conclusion remains a reasonable working hypothesis, and this is how it was presented in the conclusion section of the manuscript.
The authors however consider these arguments something more concrete than a mere speculation.

1.2) A second major issue is the lack of in situ cloud measurements and related uncertain-ties. 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.

The point was raised by all Reviewers and is discussed at the beginning of this document (Section 0.1).

1.3) 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:

1.4) - 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.

Our idea was to show how the specific set of measurements could be combined to derive, using inverse modelling in the infrared, ice clouds properties (infrared OT, and effective radius) to be used in forward computations from the shortwave to the infrared. The reference to the "test" is made in the title because of the results we obtained and our explanation for it. But the title can be changed if all reviewers agree.

1.5) - Abstract: Please explain the acronym THORPEX (even though this is well known; still, each acronym deserves explanation).....

Measurements were taken during the 2003 Pacific THORPEX (The Observing System Research and Predictability Experiment) Observing System Test (P-TOST). Will be added to the abstract.

1.6) ....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?

The statement " Cloud optical depth is also retrieved from S-HIS infrared window radiances, and it agrees with CPL values" does not mean that the retrieved optical depth in the infrared agrees with the CPL visible optical depth. The RT-RET retrieved OT is used to compute the optical depth that same size distribution and particle mixture would have at 532 nm, the lidar wavelength. The meaning of the concise abstract statement is made clear in Table 1 of the manuscript: column 4 is in fact the RT-RET optical depth computed at 532 nm. It is the RT-RET derived optical depth at 532 nm and CPL measured optical depth that are in agreement.

If the sentence "...The CPL-derived optical thickness agrees with that obtained from S-HIS, that might be a random result" refers to the fact that the fovs considered are few and the closeness in the results might be a lucky case, we note that RT-RET was validated against the AHSRL (Artic High Spectral Resolution Lidar) [Maestri and Holz, 2009] and later compared against Raman lidar data in other compaigns such as EAQUATE [Maestri et al., 2010; Di Girolamo et al., 2009] and COBRA [Maestri et al., in preparation]. In all these cases the methodology used to convert the retrieved IR OT to shortwave OTs is the same.

Maestri T., Di Girolamo, P., Summa, D., Rizzi R.: Clear and cloudy sky investigations using Raman lidar and airborne interferometric measures from the European AQUA Thermodynamic Experiment. Accepted for publication on Atmsopheric Reseach. Doi: 10.1016/j.atmosres.2010.03.020. 2010.

Di Girolamo, P., Summa, D., Lin, R.-F., Maestri, T., Rizzi, R., and Masiello, G.: UV Raman lidar measurements of relative humidity for the characterization of cirrus cloud microphysical properties, Atmos. Chem. Phys., 9, 8799-8811, 2009.

Maestri T. and Holz R.E.: Retrieval of Cloud Optical Properties from Multiple Infrared Hyperspectral Measurements: A Methodology based on a Line-by-Line Multiple Scattering Code. IEEE Transactions on Geoscience and Remote Sensing, Vol. 47, Issue 8, pp 2413-2426, doi 10.1109/TGRS.2009.2016105, 2009.

1. Introduction

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

2. Description ...

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

Pacific THORPEX Observing System Test (P-TOST).

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

The paper is already quite long and complex and the three instruments are well known and well described in existing literature (including web-based).

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

Geopotential at 200hPa and Mean Sea Level Pressure (NCAR/NCEP reanalysis (http://www.esrl.noaa.gov/psd/data/20thC_Rean/)) are shown in Figure 1.10-1. At the surface, a high pressure system extends north of the Hawaii islands, smoothly decreasing towards the equator. The analysis of the geopotential at higher levels reveals an undulation of the tropical jet stream over the eastern Pacific. A ridge extends to the west of the Hawaii whereas to the east a trough with an associated slowing down of the jet and upper-level divergence extends towards the equator. The inspection of the vertical velocities at 200hPa shows upwelling on the east side of the trough associated to the extended high cloud cover that can be observed in the GOES image (Fig 1 of the manuscript).

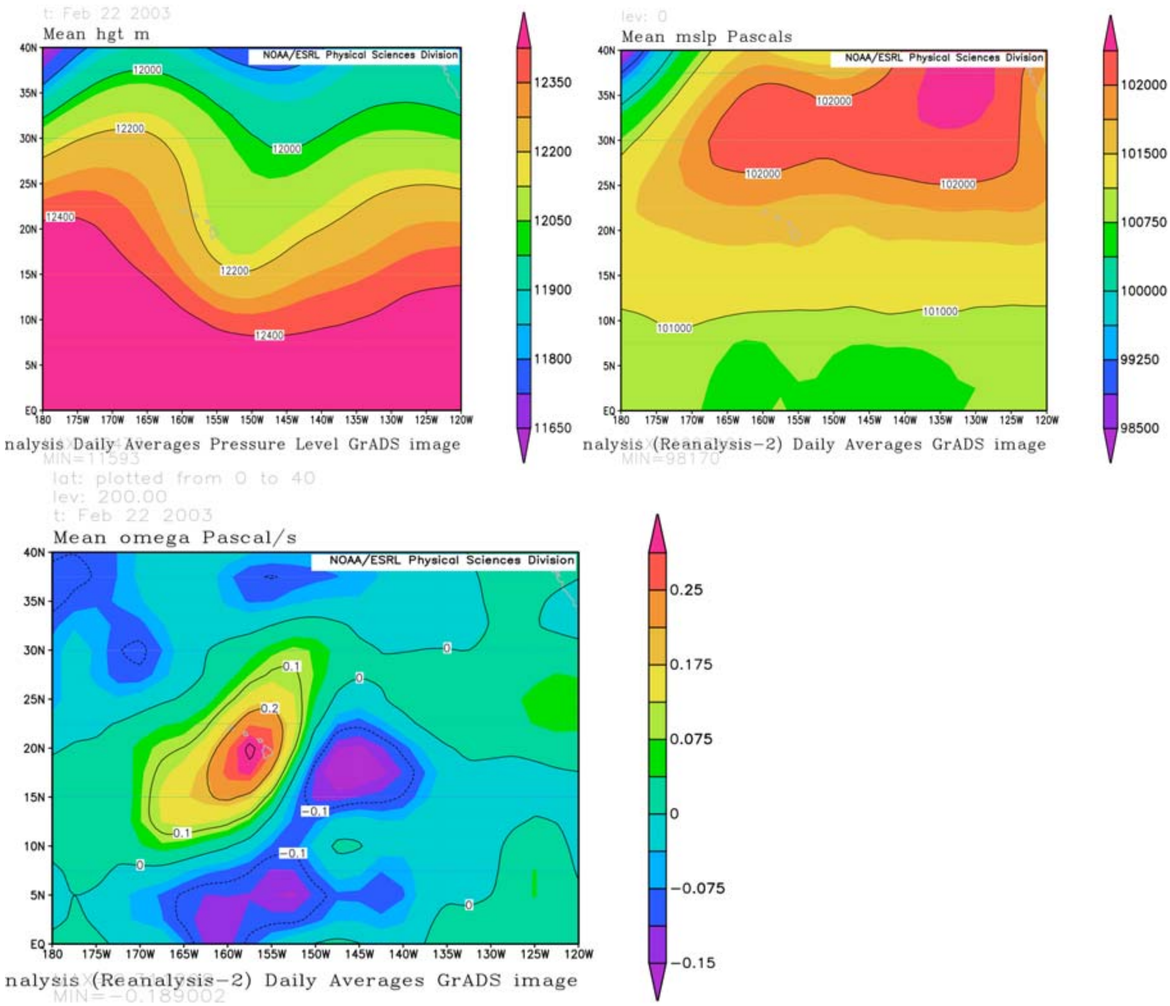

Figure 1.10-1. Top: 200hPa geopotential (left) and MSLP (right) on the 22nd of February 2003 in the eastern Pacific. Bottom: vertical velocity (Pa/s) at 200hPa

1.11) - 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.

In the early stages of manuscript preparation, the section on the post-processing required to obtain colocated S-HIS and MAS measurements was more extensive and was later reduced because the length of the paper was excessive. It is a fact that datasets can rarely be taken at face value and some re-processing is needed when data is looked carefully. The authors think these common difficulties should not be hidden, especially to the younger researchers that may read the paper.

1.12) - 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 ...

LbLRTM is a state-of-the-art line-by-line model that computes very high resolution layer molecular absorption optical depths and can also compute radiances in clear conditions, that is when there is no scattering involved. We have used LbLRTM to generate the molecular absorption database only.

1.13) - 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 ...
The quality of the figure seems quite reasonable (especially when enlarged), and we have agreed to redraw many figures that contain more important details.

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

The page numbering probably refers to the version with figures within the text, and the 3rd paragraph reads: "The integration of the radiative transfer...", that is pg 7221 line 21. Here the term "particle" refers to any particulate matter, including aerosols, ice crystals and water droplets. The scattering properties are defined for each layer and differ from one layer to the next.

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

The term applies to Fourier Trasform Spectrometers (FTS) and specifies exactly the instrumental line shape. This information is required for proper simulation of the signal measured by an FTS from high resolution radiances.

1.16) - 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.

The sensitivity test is discussed at the beginning of this document (section 0.1)

1.17) - 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.

The number of Legendre terms used can be hardly defined "very limited": in fact the authors' objective was to "minimise the number of Legendre terms required for an accurate reconstruction", and the number of terms actually employed is given in the manuscript. As an example in Fig 1.17-1 the original phase function, after truncation using Potter's method, is shown together with the one reconstructed with a number of Legendre coefficient as explained in the manuscript. The figure shows that the fractional difference is an oscillating function that is generally below 10% except at some specific angles where it can reach values between 20 and 30%. The geometry of the nadir measurement in the cloudy case is such that the (single) scattering angle is 123°.

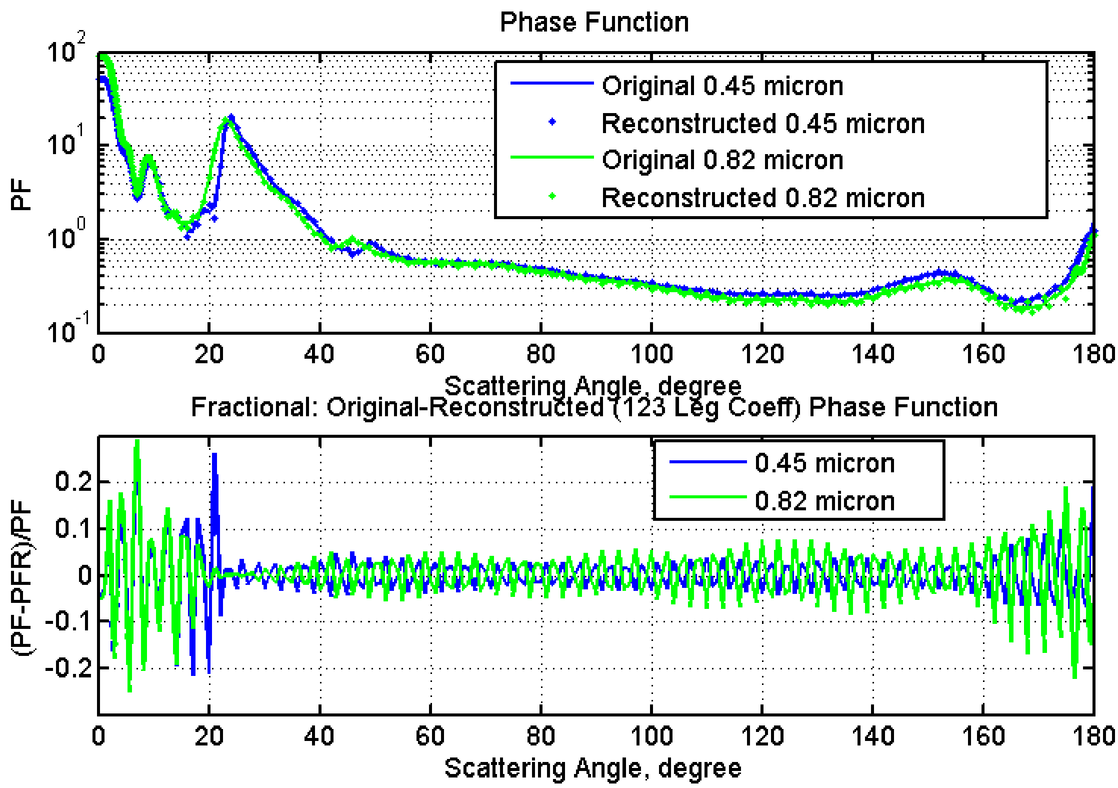

Fig 1.17-1 – Upper panel: original (PF) and reconstructed (PFR) phase functions in the shortwave, at 0.45 and 0.82 microns.; lower panel: fractional difference computed as (PF-PFR)/PF.

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

We have retained only the figure on the left to define clearly the geometry and radiance averaging involved in the cross-scan and nadir clear case comparisons.

1.19) - 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?

a) Unfortunately, MODIS overpasses were not available for the cloudy case study (01:26 to 01:34 UTC of 23/02/2003). The GOES image (Fig 1 of the manuscript) is taken at 01:30 UTC of 23/2. The closest Terra MODIS granule is at 22:25 of 22/2 (Fig. 1.19-1), three hours earlier than the case study and covers the upper-eastern part of the GOES image outside the experimental area. The closest Aqua MODIS granule is at 19:20 of 22/2 (Fig. 1.19-2), six hours earlier than the case study, and covers the the eastern edge of the GOES image, outside the experimental area.

b) The relevance of aerosol scattering is documented by Fig 5 of the manuscript, since the red curve (surface reflection and molecular scattering by atmospheric layers) is almost completely covered by the green curve (full case: surface, molecular scattering by atmospheric layers and aerosol scattering combined): that is the aerosol effect is barely noticeable.

c) We have used lidar optical depth produced by the CPL Science Team. We would have preferred to use a measured aerosol extinction profile down to the ocean surface and agree that the reconstruction of the data below 500 meters would be a serious weakness in case the aerosol optical depth were not so small.

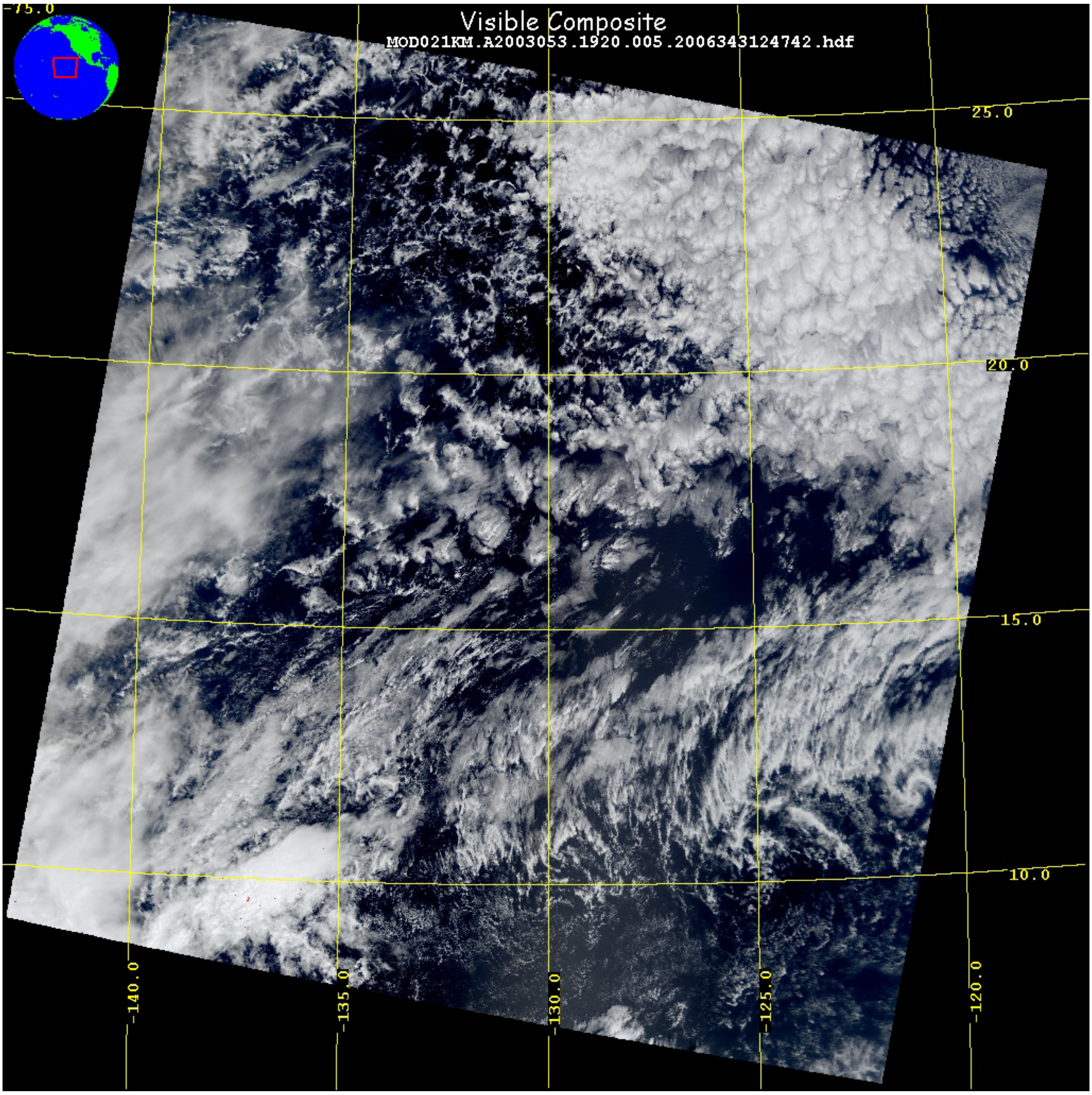

Fig. 1.19-1 – MODIS on Terra (MOBRGB.A2003053.1920.005.2006344000922.jpg)

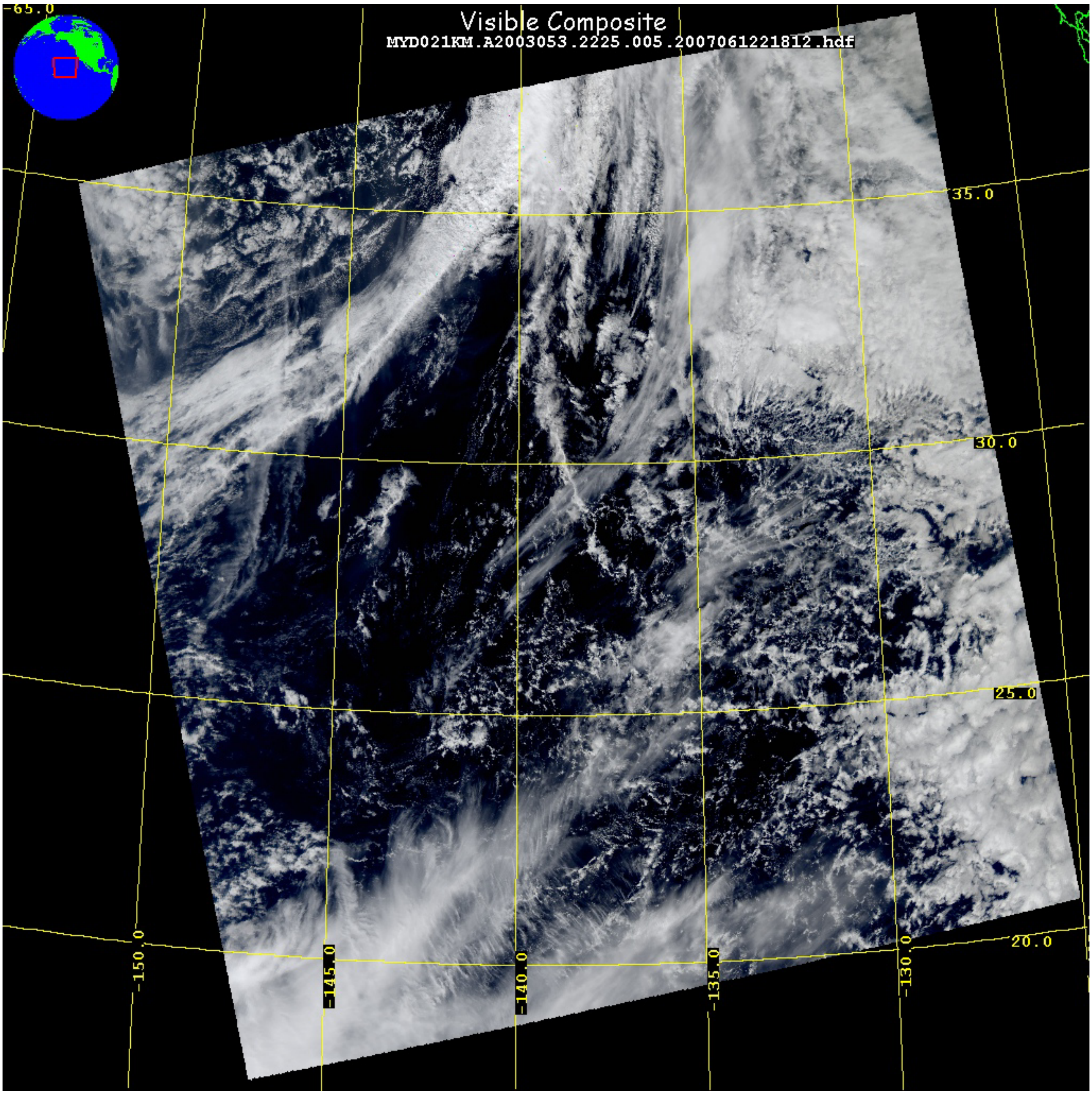

Fig. 1.19-2 – MODIS on Aqua (MYBRGB.A2003053.2225.005.2007078025756.jpg)

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

Same algorithm as used in the 6S code (Vermote et al. 2006).

1.21) - 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?

In the MAS file no uncertainty is provided for the radiance data (http://mas.arc.nasa.gov/data/flt_html/03615.html). The nominal errors associated with the MAS channels are listed in Table 2 of King et al. Table 2. For a standard scene, the declared equivalent noise in the IR channels is of less than 0.5K, whereas in the solar range is of order of 0.2 W/(m^2 μm sr) between 1 and 0.5 um and 0.02 W/(m^2 um sr) between 1.6 and 2.4 um

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

6S is a very complex radiative transfer model and interested readers should refer to the referenced manual. Briefly, as stated on the official web site (http://6s.ltdri.org/index.html), the 6S code is a basic RT code used for calculation of lookup tables in the MODIS atmospheric correction algorithm. It enables accurate simulations of satellite and plane observation, accounting for elevated targets, use of anisotropic and lambertian surfaces and calculation of gaseous absorption. The code is based on the method of successive orders of scatterings approximations and its first vector version (6SV1), accounts for radiation polarization. It was publicly released in May, 2005.

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

The red curve (surface reflection and molecular scattering by atmospheric layers) is almost completely covered by the green curve (full case: surface, molecular scattering by atmospheric layers and aerosol scattering combined) because the aerosol effect is barely noticeable.

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

The requested figure is given below: however the differences are fractional (not percentage).

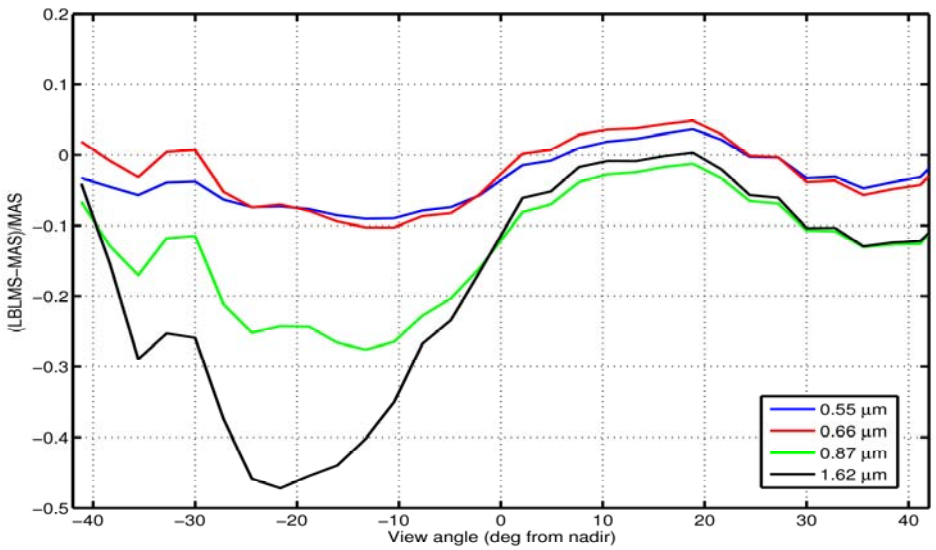

Fig. 1.24-1 – Fractional differences between LBLMS simulations and MAS radiance.

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

1.26) - 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?

Fig. 8 has been redrawn (shown below): white now refers to pixels with integrated optical depth smaller than 0.02 and black to integrated optical depth greater than 2.98, that is the black refers to layers where the signal is saturated or to field of views for which no CPL measurement is available.

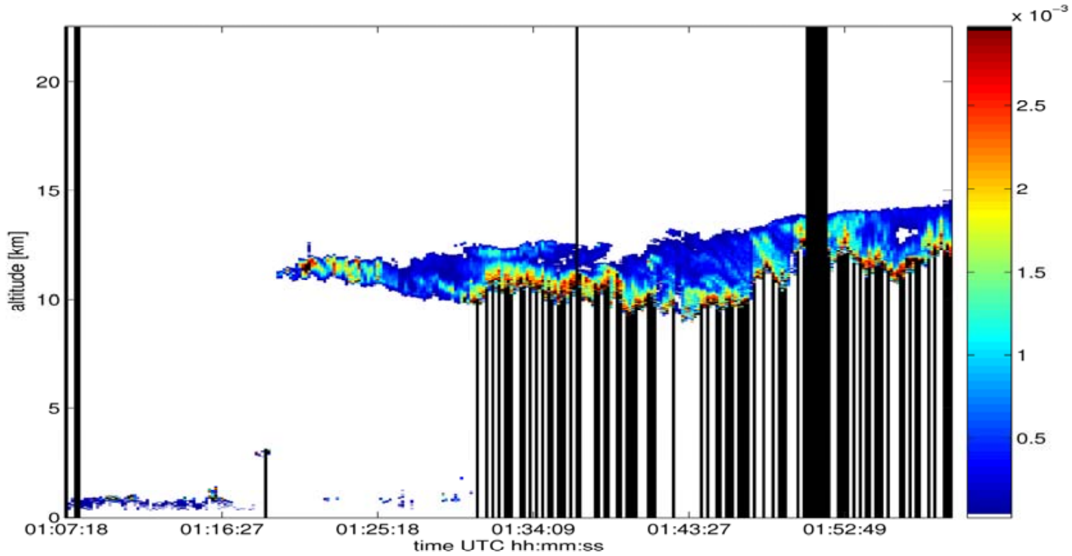

Fig. 1.26-1- CPL extinction cross section from 01:07 to 01:54UTC on the 23 February 2003.

1.27) - 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?

The choice of the cloud sectors has been done in conjunction with the MAS imagery, the result of the retrieval process and the LIDAR data. It is true that the LIDAR-based OT shows quite a variance , but this is expected given the smallness of the LIDAR field of view when compared to the other instruments: in fact the CPL delivers one data product every 200 m that represent the average of various lidar pulses (1 meter footprint for a 10 km distance between CPL and the cloud). Therefore selecting an area smooth, but extended enough, to provide a sufficient number of collocated MAS and S-HIS field of views, can be quite challenging.

1.28) - 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.

We have decided to redraw the figures without including the 1848 cm^{-1} channel. The new version is not shown.

1.29) - 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.

The role of different PSD has been clarified and is discussed at the beginning of this document (section 0.1). Unfortunately we fail to understand the meaning of '*consistency between the PSD and the OD'* and hope that the answer is already in our replies.

1.30) - 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.

The MODIS case study is important in the economy of the manuscript for two reasons:

1. it shows that LBLMS computes radiances that are close to the one measured by MODIS, when the cloud optical parameters are the MODIS retrieved ones, as discussed in our reply to (1.1).
2. it allows to identify one possible cause of the evident radiance underestimation by LBLMS in the P-TOST case study.

Our methodology should provide the best possible simulation (in a plane parallel radiative transfer), albeit applicable only to case studies. After the results of the P-TOST case, we decided to use LBLMS to simulate MODIS data *relying on MODIS cloud products*, retrieved from the MODIS shortwave radiance data. The good correspondence found between measured MODIS radiance data in the shortwave and LBLMS simulations is in clear contrast with LBLMS P-TOST simulations, and *this contrast requires an explanation* (it is indeed not a new fact, and the manuscript quotes a number of papers that have encountered similar difficulties).

The explanation, offered in the manuscript, is discussed also in our reply to (1.1). We add only that our proposed explanation involves only the structure of the phase function, not the whole 'ice crystal optical model'. We are aware that the P-TOST cloud case involves a nadir measurement geometry, and that, in single scattering conditions, our tentative conclusion regarding the phase function would apply only to a limited range of scattering angles.

1.31) - 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?...

It is the "fractional radiance difference" that is difference between simulated and measured MODIS radiances divided by the MODIS radiance.

.....In

1.32) - 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).

The percentage values in lower panels of figures 16,17 and 18 have been changed to fractional values, as requested. (Since the figures are same except for the labels along the ordinate axis, they are not reproduced here).

5. Conclusions ...

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

We have tried to clarify our standing in replies 1.1 and 1.30.

1.34) - 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?

See reply to point 1.19.

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

Anonymous Referee #2, Received and published: 19 April 2010

General comments

This is an interesting paper that utilises visible and infrared measurements of cirrus to test the physical consistency of assumed optical properties. This is an important topic of research since it is critical to show that optical parameterizations used in climate or weather prediction models have value in simultaneously predicting visible and infrared radiances that are within measurement uncertainty. This is not only important in terms of predicting the net radiative effect of cirrus in climate models but also simulating visible and infrared radiances in both climate and weather prediction models and for this latter application the phase function is critically important.
However, for the assumed ice crystal habit distribution used in the paper the authors do not find that the single-scattering properties predicted by the shape distribution are physically consistent across the near-ir and infrared spectrum, if they used the retrieved optical depth to simulate the MAS measurements. However, they find that the opposite is true if they used the retrieved re and optical depth from MODIS to simulate those measurements. The paper addresses the question as to why this contradiction might arise. The authors conclude that it is to do with the assumed phase function used in the short-wave.

The major problems with the paper are that there is no truth (i.e., in situ measurements of the PSD) or sufficient simulation experiments to exclude a number of other possibilities that might cause the apparent contradiction. Although, the paper does demonstrate the difficulty of addressing the contradiction without adequate in situ data, so from this point of view it would be of interest to publish the paper, if the following concerns can be addressed.

Major concerns

2.a. Firstly, given the lidar vertical extinction profiles shown in Fig. 8, why should the authors believe they can demonstrate physical consistency across the spectrum when the cloud they are using is demonstrated to be vertically inhomogeneous? Only if the cloud was observed to be 'ideal' in the sense that the cloud was observed to be suitably homogeneous could a test of physical consistency be performed. Or have the authors used a particular portion of the cloud that appears to be homogeneous? If so, this should be more clearly shown.

New simulations were done with a non-uniform vertical profile of ice amount, following the CPL measured optical depth profile and assuming that the PSD and the particle mixture is same in the whole cloud depth. The results are discussed in section 0.2

The authors use a sophisticated simulation tool. They find that the simulation methodology, despite their best efforts, fails at shortwave and near-infrared wavelengths. The authors attribute this lack of consistency to some basic radiative property of the assumed cloud. It is not clear why, according to the Reviewer, consistency should be expected only for a suitably homogeneous cloud. After all the distribution of mass within the cloud is same for computations at any wavelength. The computation performed for non-homogeneous cloud show that the distance between simulations with varying profiles are much smaller than the difference with measured data, so the discrepancy is not to be attributed to the assumption of homogeneity (which is, for the time being, required by RT-RET to retrieve total optical depth and effective radius).

2.b. The shape of the PSD is very important in determining the bulk single-scattering properties of the cloud. The definition used by the authors is inadequate with the parameters of the PSD poorly defined. The authors assume that mu=0 so the PSD is assumed exponential. The choice of lambda becomes critical as the lidar will become very sensitive to these small particles, after all the lidar measurement is critical to the paper. The choice of lambda must be stated together with sensitivity tests to determine

how the choice of lambda affects their results. The authors should also state the range of D the paper considers. I am concerned that the authors consider a size of zero! Since ice crystal size in cirrus does not vanish, though this assumption results in an exponential PSD that does not compare to reality and their modelled extinction will be biased to very small particles. The authors should perform a series of truncations of the PSD at the small particle end to determine the size to which the lidar becomes sensitive, and then use that size as their minimum D.

The point was raised by all Reviewers and is discussed at the beginning of this document (Section 0.1).

2.c. A further sensitivity test the authors should include is the impact of vertical inhomogeneity on their results. It is suggested the authors assume a three-layer model with the PSD from 2 above varying in each of the three layers. This variation in vertical inhomogeneity could result in differences across the spectrum due to the differing depth of penetration of each wavelength.
Since no in situ information is available the above simulations are important to perform as the forward modelling in the case of Fig. 8 is not just about the optical properties but it is also about the PSD and the vertical structure of the cloud.

The impact of vertical inhomogeneity is discussed in section 0.2.

Other comments
2.1. Title: clouds -> cloud

Done.

2.2. Introduction page 7217. Ham et al. -> Han et al and throughout rest of paper.

We have checked the paper and the reference is correct except for the volume number that will be corrected.

2.3. line 12 7217 "input of a. . ." -> 'input to a. . .'

Done.

2.4. line 15 page 7218 Were the cloud decks single layer ? with no cloud beneath and

The case study involves a single deck cloud layer, and only minimal influence from low level clouds, as shown clearly by the new Fig 1.26-1 which replaces Fig 8 of the manuscript.

2.4b ...over the sea ? If so these conditions should be stated in this paragraph.

Image information (Fig. 1 of the manuscript) and the text (page 7219 from line 1: "The PTH data-set was measured on 22 and 23 February 2003 during the flight of the P-TOST over the Pacific Ocean SE of the Hawaiian Islands") clearly indicates that the case study is over the ocean.

2.5. Line 29 7218 '..in last section..' -> 'in the last section'

Done.

2.6. Line 15 page 7222. What is the definition of effective particle size used in this paper? The definition should be fully stated and is the definition the same as that used for the MODIS retrieval? Since re is used and De elsewhere in the paper, it is important to be consistent in this regard.

The definition of effective dimension D_e of a PSD made of non-spherical shaped particles is the same that was introduced by Foot [1988]:

$$D_e = \frac{3}{2}\frac{IWC}{\rho_i \int_{D_{\min}}^{D_{\max}} P(D)n(D)dD} = \frac{3}{2}\frac{\int_{D_{\min}}^{D_{\max}} V(D)n(D)dD}{\int_{r_{D\min}}^{D_{\max}} P(D)n(D)dD},$$

In the equation *P(D)* and *V(D)* are respectively the projected area and volume of a particle with maximum dimension *D*. The ice density, ρ_i, is assumed constant with the particle's dimension. Effective radius is defined as *Re=De/2*.
The same definition is used by Prof. P. Yang and Dr. B. Baum which are the developers of the scattering database used here and which, in turn, follow King et al. [2004]

B. A. Baum, A. J. Heymsfield, P. Yang, and S. T. Bedka, "Bulk scattering properties for the remote sensing of ice clouds. Part I: Microphysical data and models," J. Appl. Meteorol., vol. 44, no. 12, pp. 1885–1895, Dec. 2005.

M. D. King, S. Platnick, P. Yang, G. T. Arnold, M. A. Gray, J. C. Riédi, S. A. Ackerman, and K. N. Liou, "Remote sensing of liquid water and ice cloud optical thickness and effective radius in the arctic: Application of airborne multispectral MAS data," J. Atmos. Ocean. Technol., vol. 21, no. 6, pp. 857–875, Jun. 2004.

J. S. Foot, "Some observations of the optical properties of clouds. II Cirrus," Q. J. R. Meteorol. Society, vol. 114, pp. 145-164, 1988.

2.7. Line 4 page 7221. The RT model is plane-parallel and assumed to be homogeneous? If so this should be stated.

The radiative transfer equation is solved for a plane-parallel geometry and the atmosphere is divided into a number of layers (81), each assumed to have homogeneous scattering properties (that change from layer to layer).

2.8. It is stated on page 7225 line 1 that the channel centred on 1848 cm-1 did not work properly, this being the case why is it included in Figure 3 ? as this contradicts the main statements given in the text of the paper describing figure 3.

Agreed. The figures have been amended to eliminate channel at 1848 cm-1 (non shown in this document).

2.9. On page 7234 Table 5 is mentioned, do the authors mean Table 2?

The reviewer is correct: the numbering was erroneously provided by LaTex and still we do not understand where the "5" is coming from.

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

Anonymous Referee #3, Received and published: 14 May 2010

General comments: This paper shows comparisons of modeled radiances in the visible and near infrared region against measured radiances from MODIS Airborne Simulator (MAS), Scanning High-resolution Interferometer Sounder, (S-HIS), and Cloud Physics Lidar, (CPL). In general the paper is well written and highlights potential inconsistencies in the ice crystal phase function models between the SW and NIR spectrum. It is not obvious however, that the purpose of the paper is to 'test' ice cloud optical properties, in fact the paper is organized more as an evaluation of the forward modeling methodology (LBLMS).

The juxtaposition between the MAS cloudy case and the MODIS cloudy case is a little confusing. I think there needs to be a better explanation of the purpose of section 4.3 in both the introduction and in section 4.3.

3.1) Explain why the that particular field experiment data were chosen, why not use data from an experiment where cloud in-situ data were available? Same thing for the MODIS case, why not choose a granule where more coincident data sets were available, for that matter why not use a MODIS Aqua granule and collocated CALIPSO/CloudSat data to get accurate cloud heights and water phase.

A) The P-TOST campaign was designed specifically as a fine tuning opportunity of the MODIS and AIRS science product algorithms. Our decision to use the P-TOST data was taken shortly after the experiment was completed, and the choice was based on the following elements:

1. high spatial resolution radiometric data spanning a spectral interval from shortwave to infrared;
2. high spectral resolution infrared interferometric data colocated with 1.;
3. a clear sky and an ice cloud scenery within the same mission;
4. detailed measurement of the atmospheric profile of temperature and humidity;
5. availability of the airborne LIDAR (on same platform as 1. and 2.) to provide aerosol products for the clear case and cloud products for the cloudy one.

At that time a purely satellite campaign could not have the same characteristics, especially the high spatial resolution imagery through the whole spectral range from the IR to the visible. We agree that the specific campaign would have greatly benefited from a satellite overpass but unfortunately it wasn't the case (see also our reply to point 1.19-a).

For the clear sky case the case study provides an interesting combination of wind pattern and sun glint to test the surface reflection properties of the modelling suite.
We agree that additional in-situ sampling of the microphysical features of the cloud layer would have been key observations but, to the best recollection of the authors, there were no dataset available at that time with the above characteristics combined with in-situ microphysical measurements. In fact some of the authors had already experience with microphysical cloud data combined with high resolution infrared radiance data (Maestri et al., 2005; Rizzi et al., 2001).

We are not aware of datasets available today that combine in-situ microphysical data and all the elements listed above .

Rizzi R., Smith J.A., Di Pietro P. and Loffredo G., Comparison of modelled and measured stratus cloud infrared spectral signatures, J. Geophysical Research, 106, D-24, 34109-34119, 2001.

B) The MODIS case is studied to understand the reason behind the evident radiance underestimation by LBLMS. Let us point out that our methodology should give us the best possible simulation, albeit applicable only to case studies. We decided to use LBLMS to simulate MODIS data *relying on MODIS cloud products*, that is retrieved from the MODIS shortwave radiance data. The good correspondence found between measured MODIS radiance data in the shortwave and LBLMS simulations is in clear contrast with LBLMS P-TOST simulations, and *this contrast requires an explanation* (it is in fact not a new fact, and the manuscript quotes a number of papers that have encountered similar difficulties).
We could have used a MODIS Aqua granule, but still we would have relied on MODIS products because our P-TOST results were based on accurately measured cloud heights (a CPL product) and in any case our simulations were providing ample evidence that the problem was not due to inaccurate cloud heights (and water phase was not an issue

for our case study).

3.2 There should be more discussion on the parameters of the particle size distribution used in the specific simulations.

Discussion on the PSD parameters is now given in section 0.1 of this document

Specific comments:
3.3) Section 4.1: It be nice to see the impact on the simulations if the CPL extinction data were used to infer the vertical distribution of IWC. There are certainly going to be differences in the IR radiances for an inhomogeneous vertical distribution of cloud extinction. Does the RT-RET retrieval use the CPL cloud optical depth or the extinction profile?

RT-RET assumes homogeneous cloud properties (ice content, size distribution and effective radius) within the cloud and retrieves the OT (integrated optical depth) and effective radius. In the forward simulations we have used an homogeneous cloud to be compatible with the retrieval methodology. The point raised by the reviewers is discussed in section 0.1.

3.4) Page 7226, sentence starting on line 15. Why not use the best 'assumed atmospheric temperature profile', the reasoning hear seems weak.

The 'best assumed atmospheric temperature profile' could be defined and used, but in this case study we were not particularly concerned with the small deviations in the infrared range and we did not optimise those computations. Pointing out the problems and possible improvements is however, in our view, not a wrong attitude.

3.5) Page 7237, sentence starting on line 13. This is a false statement, the lidar retrieval of optical depth depends on the value of the phase function at 180 degrees (the backscatter phase function). No where is it stated in the text if the lidar retrievals are assuming a default backscatter phase function, or are using a measured value.

The S-ratio is defined in McGill et. al (2003) as "*the total scattered and absorbed energy divided by the amount of backscattered energy*". In same paper, that deals with the use of the airborne CPL in a field campaign, it is stated: "*Under certain favorable circumstances, specified below, the S-ratio can be estimated directly from the lidar data without assumption, but more often external information will be required". The method to estimate the S-ratio is called the "transmission loss method". The 'favourable' circumstances are described as: "This approach is practical only for a cloud or elevated aerosol layer that is optically thin with either a lower layer or the Earth's surface sensed below it and has enough clear air (no aerosols) immediately below the layer to determine signal loss through the layer. The clear air zone must be at least a minimum thickness (around 0.6 km) and analysis is usually restricted to 3 km thickness. <u>Ice clouds above 5 km are the most likely candidates for this analysis</u>, although elevated aerosol layers with enough clean air below are also appropriate. Under these conditions,... an estimate of effective optical depth for the layer, can be determined.*"

We have assumed that in our cloudy case study the conditions for using the transmission loss method were met and we thus wrote "generally no assumptions on the phase function is required in the retrieval procedure". We can be more explicit in the final version.

McGill, M.J., D.L. Hlavka, W.D. Hart, E.J. Welton, and J.R. Campbell, "Airborne lidar measurements of aerosol optical properties during SAFARI-2000," Journal of Geophysical Research, 108, doi: 10.1029/2002JD002370, 2003.

3.6) Figure 2. Not really useful.

We have retained only the figure on the left to define clearly the geometry and radiance averaging involved in the cross-scan and nadir clear case comparisons.

3.7) Figures 3 and 4. Don't include the data from 1848 cm-1, it only draws a reader to focus on it. Just leave the statement in the text that the channel was excluded.

Agreed. The figure has been changed (but not shown here).