**Author reply to referee 3** AMT-2016-123

Dear Reviewer,

thank you for your support and for your suggestions for improving our manuscript.

In general, all reviewers suggest to strengthen the literature review, especially to improve the discussion of earlier publications on imaging ice cloud remote sensing (Schaefer et al. 2013) and the introduction of the visible spectral slope solution for the transmissivity ambiguity (Brueckner el al. 2014 and Le Blanc et al 2015). This is an obvious weakness of our manuscript. The reason for this negligence on our side is partly due to the fact that our manuscript has had a long history already. In our group the spectral slope approach originally goes back to a Master's thesis of co-author Petra Hausmann from 2012. We obviously noticed that "our approach" was published meanwhile in proper journals by others. Even though this is no excuse for gaps in our literature review, it might explain why we do not want to state any direct "use" or "application" of ideas introduced by the aforementioned authors. In our revision we do both, we try to strengthen our literature discussion, and at the same time we would like to include the Hausmann Master's thesis from 2012 as a reference. Although it is no peer-reviewed publication it is an official university thesis in English language available online.

Point by point reply to all major comments (all minor were considered as suggested):

Sequential major comments:

P2, L27: King et al. (2004) is the wrong reference for the adaptation of the Nakajima/King algorithm to ice clouds. At this point, the MODIS retrievals were already fully operational. One of the Platnick references is probably more appropriate; the cited paper is for the retrievals over bright surfaces, an entirely different problem. A nice review of ice cloud retrievals (with emphasis on the problematic phase function) is provided by *Wang et al.* (2014).

> => I once more tried to pin down the best "original" publication for our statement that Nakajima+King „can be adapted to ice". Even in the MODIS ATBD this King et al publication is listed at this point. We now mention Baum et al. 2000 on MODIS airborne simulator and ice properties which is slightly older and maybe more general. We also added the aspect of TIR cirrus remote sensing mentioning Wang et al. 2011 to the introduction. Thanks for pointing out.

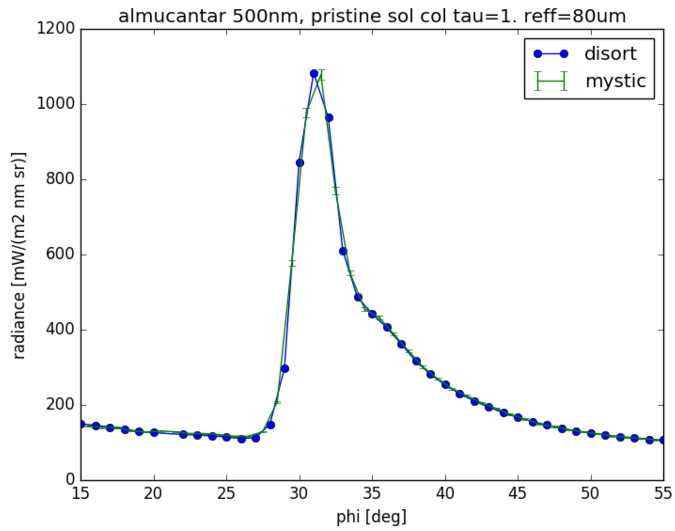
P3, L16: Clarify (here or later) that *LeBlanc et al.* (2015) used the spectral slope of normalised mid-visible transmittance (or radiance), and that this is what the algorithm in this manuscript relies on as well (rather than using the *combination* of the spectral slope and imaging capabilities, as the subsequent sentence implies). See also the comments on pages 1 and 2 of this review. Also change this appropriately at later occurrences (for example in the summary); it should be stated that the idea for resolving the ambiguity problem was first introduced by the 2014 and 2015 papers.

> => The section in the introduction now reads:
>
> Recently Brückner et al. (2014) as well as LeBlanc et al. (2015) presented similar solutions for unambiguous retrievals of optical thickness and effective radius for pointing system without providing imagery. Both suggest the use of spectral slopes in the visible to separate between the two optical thickness regimes. We will present a combination of both, a solution for the transmittance ambiguity using a similar spectral slope (following

ideas of Hausmann, 2012) and results for imaging measurements which provide context information on the distribution of optical thickness and effective radius over a large area.

P4, L24: 16 streams seem too few to properly resolve the features of the scattering phase function unless the cited intensity correction works properly for small and medium optical thickness values (single or low-order scattering) – this is of course especially true for scattering angles near the halo angles or other pronounced features. It would be highly recommended to show a plot of simulated downwelling radiance as a function of viewing angle such that the ability of the RT code to simulate the halo in the right place becomes credible. Too few streams in the solver may misplace the halo despite the application of the intensity correction. Perhaps such a plot could be added in an appendix?

=> We have been aware of this pitfall. We think that we are able to resolve the phase function correctly. This image shows a simulation of the 22° halo for pristine solid column ice particles simulated for an almucantar scan of a sun-photometer using an exact method (MYSTIC Monte Carlo) and 16-streams libRadtran DISORT.




Nonetheless we in general do not suggest the retrieval for scattering angles around the 22 and 46° halos, because of the variations of the intensity of such features due to crystal shape assumptions (and crystal roughness). With an imager we are almost always able to avoid this region and our example applications do not include such geometries.

I would prefer not to show a plot similar to the above in the manuscript, as it is a rather technical aspect and we avoid these scattering angles anyway.

We extended the final discussion of these points:

Of course the most important step forward would consist in a reduction of the crystal type uncertainty. The halo regions around 22° and 46° scattering angle were avoided here for our spectral approach. Uncertainties can be expected to be higher in these regions with strong angular gradients of transmittance under single scattering conditions, if no additional information on crystal habits is available. However the imaging capabilities of the specMACS sensor (especially if combined with a scanning platform, see Ewald et al., 2015) do not only allow to successfully avoid these regions for the spectral evaluation, but would allow for the utilization of the spatial distribution of transmittance in these regions to provide the missing information. Use of this spatial distribution could provide important constraints regarding the present average phase function as Schaefer et al  (2013) demonstrated. Especially the presence of optical scattering phenomena like type and intensity of halo displays could be used to identify specific particle shapes and orientation and information on the mixture with less perfect rough ice particles. A combination of the presented method with additional information of this kind will be the next step in our effort to provide better ice cloud property observations.

Section 2.2.2, optical properties: Using all six habits from "HEY" may not be appropriate since some of the habits are highly unlikely to occur in the study region. The authors do mention that the errors due to crystal shape represent an upper bound for this reason, but even that may not be correct if a certain habit does not occur at all in the kind of cloud that is observed. This point, combined with the previous one (are the features of the phase functions are resolved by the intensity-corrected 16-stream radiances?) cast some doubt on the concluded magnitude of the bias due to unknown habit.

=> As the true ice particle habit is never known, there is no other thing we can do but discuss maximum errors? Also we are far from identifying typical habit mixtures or typical crystal roughness for specific regions of the world. Even the long established mixtures like the Baum mixture are about to be abandoned (e.g., MODIS group) in favor of simple single habits with added roughness. So I do not see an easy way to improve the situation, before methods similar to the Schaefer et al one are developed for robust operational use. We extended the discussion along this line (see above).

Section 2.2.3, surface albedo: MODIS surface albedo products are known to have problems in regions with pronounced topography. Given that, why was the sensitivity to surface albedo and its uncertainty not tested in similar ways as done for the other parameters (see table 1)? This seems even more important than testing the sensitivity to the presence of aerosols.

=> Correct comment. Originally we did not test albedo, because we use a MODIS albedo product for the time of measurement. We now included a test of the sensitivity towards inaccuracies there with a second spectral albedo set. We added text in the sensitivity test section:

As an actual spectral albedo is averaged in a MODIS product over 16 days including the measurement period, the correct albedo influence should be represented in our forward simulations. Still uncertainties arise from the products uncertainty, the derivation process of the albedo data for the Zugspitze area and vegetation changes during the period. As test case, a second albedo data set for our main measurement site in the center of Munich city area is used. It is shown in lighter colors in Fig. 2. In the urban area, the vegetation peak between 750 nm and 1400 nm is much more pronounced (Munich is a green city), while the changes at shorter and longer wavelength are smaller. Nonetheless, the Munich albedo data set is also brighter in the regions used for the retrieval around 550 nm and 1600 nm by 15 and 20%. This increase is comparable to the difference between summer and winter for vegetated surfaces and is, at the same time, much larger than the estimate of the albedo product uncertainty (see Moody et al., 2005). Errors and uncertainties caused by such a variation of albedo are, nonetheless, small for reff (bias at 0.3 an RMSE at 4 um) and tau (no bias, RMSE at 0.5).

P6, Eqn (2) This parameter is equivalent to $\eta_{11}$ from *LeBlanc et al.* (2015), except that they used radiance (not transmittance) to derive it, which is conceptually the same – see also previous comments.

P6,l24: "Consequently, it is *now* possible to…" See comments above (origin/genesis).

=> See reply above. The word „now" was removed and a hint to the Brueckner and LeBlanc publications is left in the end of our description.

P7,l10: Why does the LUT only include three different relative azimuth angles? Can this be justified by the geometric arrangement and a special sun-viewing geometry? The relevant angle for near-single scattering remote sensing (i.e., up to about 1 optical thickness) should be the scattering angle, not the viewing zenith + azimuth angle, correct? This may only be a minor point – however, typical LUTs usually feature more than the azimuth angles used in this study, so the question is why this study can get by with so few. A plot that shows the dependence of the radiance on viewing azimuth angle for a small optical thickness would help giving some credence to the approach. Combined with an earlier suggestion: The approach can only be regarded as sound if the halo (or another feature) can be simulated in the right place (in the 2D image) and resolved both in viewing zenith and azimuth angle.

=> Before measurements the alignment of the instrument was carried out in a very simple fashion. The sensor was leveled pointing into zenith and a simple sun shadow system was used to fix the position of the spatial line perpendicular or in parallel to the solar azimuth. Thus only these geometries could appear in our measurements. New text in that section:

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Since the sensor was aligned with respect to the sun before each
measurement, a very limited set of viewing zenith and azimuth
angles has to be provided in the lookup table. ….
… Three relative azimuth angles are taken into account, because the
sensor's spatial line was either aligned parallel or perpendicular
to the principal plane: phi_rel = 0° and phi_rel = 180° (line
parallel) or phi_rel = 90° (perpendicular).
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P8,Eqn 3: It is unclear why the three terms (T550, T1600, SVIS) are each given equal weight in determining dT. After all, the measurement uncertainties propagate into each of these in different ways, and the individual terms should be weighed by their (different) uncertainty, rather than applying a global weight for the different LUT points as proposed in Eqn 5 later on.

=> The overall range of values is similar for all three parameters. A global rescaling would not change much. We have originally tried a re-scaling of the lookup table using local gradients of measured parameters with changing reff and tau and the use of a dT normalised this way. According to our sensitivity test suite, the improvement of result accuracy was similar to the one that we could reach by increasing the density of points in the lookup table. For the sake of simplicity we decided to use slightly more computational effort and abandon the more sophisticated version.

P8/P9: Related to the comment above: How is the uncertainty in COD and REFF derived from the uncertainty in the measurements (the radiances)? dT cannot be regarded as a metric for the uncertainty because it is a merely technical quantity which would change for a different LUT gridding. Also, trading standard deviation for uncertainty as done on p12,l20-21 does not help. How do we know for certain that the error does not exceed the standard deviation? Particularly for small crystal size (on the order of 10-20 micron) and the optical thickness range shown, the effective radius may be highly uncertain because of the issues described by *McBride et al.* (2011). An error propagation analysis from the radiances to the retrieval parameters should be performed, not in terms of the LUT gridding, but in terms of the underlying physics. This is probably the most work-intensive revision that is suggested in this review.

=> Right, such a complete error propagation analysis would be very valuable. Unfortunately it goes beyond the effort we can provide for this manuscript at the moment. We strengthened the discussion of your points and replace the term “quality“ by “significance“ in this context.

New text were the parameter is defined:

Significance 1-deltaT / deltaTmax=0 is related to the maximum search radius, while larger values are related to better matches and a perfect match would be significance 1.The term significance is chosen because the distance to the tabulated values, strictly speaking, is not a measure of retrieval accuracy or quality, but rather a technical quantity. Given the fact that we expect matching measured and tabulated values, if we have considered the influence factors correctly, deltaT is an important parameter which indicates the applicability of the chosen lookup table to the real measurement situation and consequently the reliability of the results. Still this parameter could be small for the wrong reasons, e.g., if impact of wrong albedo and wrong ice particle habit compensate each other.

New text in the discussion of October 3 results:

Retrievals have been possible at retrieval significance values close to 0.75 and above throughout the largest part of the scene. Following our definition of this value this means that for all pixel measurements of spectral transmissivity tabulated values are consistently within deltaT=0.025 and 0.01 of the tabulated lookup values‘ surface (compare Fig. 5).The real situation seems to be realistically represented by the lookup table.

P10,L9: See earlier comment concerning the azimuth vs. scattering angle. Azimuth does not seem to be the appropriate parameter to consider for singe-scattering conditions.

=> As mentioned in the presentation of the method there is only one set of lookup tables for a given measurement situation which has to b used, because the relative azimuth is fixed. Then the set of lookup tables with most comparable zenith angles is selected and interpolated using the scattering angle only. So you are probably right, using scattering angle as main parameter for the lookup tables might be more efficient. But first, we are sure that results would be the same (due to the last interpolation step) and second, we do not want to limited ourselves to a single scattering regime here.

P12,l20-21: See earlier comment: The standard deviation cannot replace a physics-based error analysis. Also: The impact of surface albedo uncertainty should be part of the retrieval error, unless it is deemed to be negligible.

=> See our earlier replies.

P10,l22-23: Would it help to limit to such habits that can realistically occur for the type of cloud under consideration? What about the distinction between habit and surface roughness (which seems to have a larger role than the habit when it comes to smooth versus "featured" phase functions)? Is it possible to say something about the roughness using the imaging capabilities of the instrument (in the vein of the Schäfer paper)?

=> As said before, I think there is no way to limit the habit possibilities beforehand with a spectral method only (as presented in this paper). But you are right, a further development of ideas like the Schaefer et al methods using the spatial distribution of transmittance over a wider range of scattering angles would make this possible and is definitely among our goals going beyond this paper.

P14,l16 & l18-20: Please consider earlier comments regarding the origin of the "*new* third parameter" for the removal of the ambiguity.

=> We mention Brueckner and Leblanc once more.

P16,l1-5: I commend the authors on tackling the arduous task to reduce the retrieval uncertainty due to crystal shape using spectral imaging!

=> We are working on it! For a further publication.

**References:**


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