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Interactive comment on "Optical property retrievals of subvisual cirrus clouds from OSIRIS limb-scatter measurements" *by* J. T. Wiensz et al.

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Dear Editor:

Please find enclosed our revisions to the manuscript "Optical property retrievals of subvisual cirrus clouds from OSIRIS limb-scatter measurements", which we are submitting for publication in Atmospheric Measurement Techniques. Please note that we have chosen to change the title of the manuscript to "Retrieval of Subvisual Cirrus Cloud Optical Thickness from Limb-Scatter Measurements".

The reviewers' original comments are shown in italic typefont, with our responses in standard font.

If we have not properly understood or addressed the reviewers' comments, please let us know as soon as possible so that we can address their concerns. We welcome any responses to these comments and appreciate the many helpful comments and critiques.

Anonymous Referee #1 General comments:

The article by Wiensz et al. describes an interesting and new development of a forward model for the retrieval of optical properties of cirrus clouds from the OSIRIS limb scatter measurements. AMT seams the right journal to publish this new approach and model development. The paper is generally well written but lacks at various places a more precise and detailed description on the basis of the applied methods and what is really achieved with the current retrieval approach. For example:
a) The reader can not really judge if the forward model is really doing a correct job,because no comparison with an established model is presented or alternatively a comparison/validation of the retrieved quantities with complementary instrument data has been performed.

Comparison with an established model is recommended. We would respond by asserting that the model used in this work (SASKTRAN) is a well-established model and has undergone significant comparisons with other spherical radiative transfer models (Bourassa et al, 2008 when used under clear-sky conditions). In addition, this model has been used extensively in trace gas and aerosol retrievals (multiple published results, some listed at P5315 L28ff in discussion manuscript). Preliminary comparisons have been done with aerosol and ozone measurements made by coincident SAGE II measurements. In these comparisons the combined cloud and aerosol extinction amounts retrieved from OSIRIS have shown good agreement with SAGE II measurements. Comparisons of retrieved O_3 – when concurrently retrieving cloud properties – show improved agreement with SAGE II O_3. Since comparisons were done on a selected number of scans rather than over the total available period due to time constraints, and since such a study so would significantly increase this manuscript's scope, it was decided not to include these comparisons.

b) The final results (e.g. Fig. 16) give the impression, that the extinction retrievals are not really reliable, because the measured radiances are not very well reproduced depending on the chosen effective radius of the cloud particles, even at the two selected wavelength regions for the defined measurement vector, although the retrieved measurement vector looks fine. This is very confusing and further explanations are necessary to convince the scientific community that this is a sophisticated study of an optical parameter retrieval (see details in comment P5333L16ff).

The retrieval presented in this work is primarily a retrieval of subvisual cirrus optical thickness rather than the extinction profile. The greatest sensitivity of τ_c is to the assumed particle size. This has made more explicit in the manuscript by quantifying the uncertainty in the retrieved scene albedo. Please see the response to comment P5333 L16, which provides quantitative information about the accuracy of the scene albedo retrieval.

In summary, the paper has to present more stringently what can be achieved with the current retrieval, which parts of the sensitivity study are important for the current optical and which parts for future micro-physical parameter retrievals (e.g. R_{eff}). The great potential of such a R_{eff} retrieval becomes obvious in the presented study. My suggestion/impression is, that the paper describes a new forward model development and the sensitivity (feasibility) study of various parameters of interest (optical and micro physical) as well as secondary parameter like the described albedo effect, which can be

used for a scientifically profound retrieval approach. A retrieval study would definitely need some substantial validation section which is missing here. The authors should comment on how they like to validate or have already validated the forward model in the manuscript and how reliable the extinction retrieval results are. Depending on the changes and improvements of the revised manuscript you may change the title of the paper and structure of the manuscript.

We have changed the title of the manuscript to 'Estimation of Subvisual Cirrus Cloud Optical Thickness from Limb-Scatter Measurements', and have highlighted the fact that this work is primarily a retrieval of optical thickness and is intended to be built upon in a more detailed study.

Specific comments:

Abstract:

a) *Please note which optical properties you like to retrieve in the study.*

The changed title is also reflected by this section of the abstract. It is an optical thickness retrieval for an assumed effective particle size.

b) *The term 'accurately' is not quite quantitative. It might be problem of the recent version of the article that a quantitative error budget isn't achievable. However, the author should address a more objective estimate of the potential errors of the forward model and the retrieval quantities.*

This sentence has been rephrased in the manuscript to describe the primary sensitivities of the optical thickness retrieval: "it is shown that the retrieved extinction profile for an assumed effective cloud particle size models well the measured in-cloud radiances from OSIRIS. The greatest sensitivity of the retrieved optical thickness is to the effective cloud particle size."

P5315 L5: *For completeness, please notice that measurements of SVC have been also made by IR limb sounders (CLAES, CRISTA, MIPAS) with similar detection sensitivity and add references.*

Measurements with references added to manuscript.

P5315: *Generally, acronyms have to be explained when mentioned first, here e.g. SAGE II, CALIPSO, CPI, Odin.*
Clarified in manuscript. Odin is the name of OSIRIS' host satellite and is not an acronym.

p5316: *'modelling thin cirrus observations' please specify: optically and/or vertically thin?*
Optically thin. Changed in manuscript.

P5317 L7: *Is the Θ=380K an objective criterion for the tropopause, please specify and/or give a reference.*
For SVC retrievals in this work, we focus on measurements made at latitudes less than $25°$. In this region, this level serves as a more helpful upper bound to the tropopause height (Holton, 1995) than the thermal tropopause due to disturbances from deep convection in this area. See also response to comment P5330 L14.

P5317 L17: *What's 'odin', please clarify the acronym.*
Please see response to comment P5315.

P5318: *please note that the Baum et al. optical properties are bulk properties for fixed cloud compositions. Are these compositions in opposite to the new findings of the Lawson et al. results mentioned in section 1?*
The Baum et al. compositions are not in conflict with the new findings of Lawson et al. Quasi-spherical shapes (droxtals) are dominant for particle sizes $D < 60$ μm. Hexagonal columns and plates dominate for $60 < D < 1000$ μm. This has been noted

in the manuscript.

P5318 L22ff: *The discussion of the field of view, vertical sampling and consequences respectively the 'reasonable physical assumption' drawn from this are not well presented.*
(a) Is the 1km FOV an estimate of the FWHM or the total FOV?
(b) The vertically sampling seems larger than the FOV, but the sampling volume should be independent from the vertical sampling. A FOV of 1km is already a very good resolution for limb, why you conclude that R_{eff} and number density vary only with height is a reasonable assumption for modelling the OSIRIS data. This might be correct but is not obvious from the numbers you presented. Which other variability is neglected (horizontally)?
(a) The 1km is the total vertical FOV.
(b) A main reason for this discussion was to state that typically only 1-2 exposures pass through an SVC region per OSIRIS scan. From a retrieval perspective, it seems to be an unnecessary complication to assume a range of effective sizes over the height of the cloud. The sizes of ice crystals clearly change throughout the vertical profile of the cloud (assuming a very simple, 1-dimensional microphysical picture of SVC maintenance and formation), but a variation in particle sizes is accounted for by the underlying particle size distributions. We have found that varying the effective size throughout the cloud layer has minimal effect on the modeled radiances for the small number of samples per cloud.

P5319 L4: *Are there any polarisation effects in the measurements?*
OSIRIS measures only unpolarized radiances, and polarization effects in the instrument diffraction grating are accounted for in the radiometric calibration.

L8: *Please specify why thermal trace gas emissions are negligible in the signals of interest. These might be modified by the cloud scattering effects.*

We have chosen wavelengths for the measurement vectors that lie outside significant trace gas absorption or emission features, so it is not expected that this is an issue.

L11ff: *Please explain all formula symbols ω, P. It seams to me this paper is missing in some parts a carefully proof-reading.*
Single-scatter albedo and phase function definitions clarified in manuscript.

P5320 section 3.3: *This section is too much condensed. It is not really traceable why the combination of methods (minimum energy distribution, novel photon conversation technique and transport approximation) are important and necessary in the forward model. Please present more details that non-experts can follow your arguments (e.g. why is the extremely sharp peaking phase function a 'problem'?).*
This section has been expanded for clarity in the manuscript by noting the specific discretization of the scattering integral. The discretization leads to large numerical errors when scattering occurs by typical cirrus-cloud particles, which was treated in detail in the Wiensz et al paper cited.

P5321 L25: *Please specify optically or vertically thin.*
Sentence has been removed following the addition of clarifying sentences due to comment P5322 L1.

P5322 L1: *Please give a reference why the PSD of SVC is well described by uni-modal gamma distributions and not by log-normal distributions like suggested by Tian et al. (J. Atmos. Sci., 2010).*
The authors were not aware of the work by Tian et al. According to the authors' knowledge, the Baum database provides the best available estimates of cirrus cloud properties. This database assumes a unimodal gamma distribution. The Tian et al. work is now referenced in the manuscript: "Simulated cloud layers in this work assume the

optical properties from a single effective particle size D_e throughout the vertical extent of the cloud using the properties of the Baum et al database, which use a unimodal gamma distribution (Heymsfield et al, 2002). Is should be noted that recent studies (Tian et al, 2010) have shown that the particle size distributions in cirrus are well-described by a lognormal distribution, however this difference has minor implications for OSIRIS SVC measurements." This is taken to be the case for the reasons listed in the response to P5318 L22ff (b).

L6: *Later on you retrieve only τ_c or extinction from the measurement vector. But for me it seams obvious that the location of the cloud in the FOV is also a sensitive parameter for the retrieval. Please specify more clearly which simplifications you apply for the retrieval.*
The following has been added to address this comment: "For the forward model study in Section 4 a modeled cloud is then characterized by the parameters h_{ct}, Δh_c, D_e, and τ_c. When this model is applied to retrievals of cloud extinction in Section 5, the cloud top altitude and cloud thickness are implicitly found through the cloud particle number density profile."

L16, L26 and following pages: *It is a bit confusing that the sensitivity tests on the 'diffuse point spacing' , SZA, and albedo effects are always with varying cloud thickness, R_{eff}, and τ_c settings. This makes it very difficult to compare the different effects. Is it possible to homogenise the parameter settings (e.g. sections 3.7 (2), 4.1, 4.3)? Please, try to make the different sensitivity test comparable.*
The revised manuscript now shows the modeled results using homogenized cloud properties, as suggested, using the modeled cloud layer from Fig 5a.

P5323 L23ff: *I can not follow the arguments of the authors why vertical thickness and cloud top altitude are not an issue for the sensitivity tests. I guess the authors have investigated this point, then a short section should highlight the results. If this is not the case, a more detailed argumentation must be presented to convince the reader about this exclusion.*

A short section showing the variability has been added.

P5324 L6: *How you estimate the cloud thickness of 300m, from the measurements? Please specify.*
The thickness is simply a reference thickness used for the modeling study, and one which is compatible with SVC vertical thicknesses between 300m and 1km.

L7ff: *Is the argumentation about the ideal stabilisation conditions in this part of the manuscript really helpful? If I remember correctly the Luo et al. (2003) work is focussing in ultra thin tropical cirrus (UTTC) and not SVC in general.*
In order to homogenize the cloud settings for the model study (see comment P5322 L16, L26) a different scan was used in the revised manuscript. The reference to the Luo paper has been removed.

P5325 L8: *'Woods anomaly' please describe this term in more detail for interested but non-expert scientists. It seams an important issue for the retrieval error budget.*
Clarified in manuscript: "The notable decrease in measured radiance above 795 nm is likely due to the uncertainty in the preflight radiance calibration of polarization effects in the grating – the Woods anomalies – which affect the measurement uncertainty due to the absolute calibration of the spectrograph."

L14ff: *'As the cloud becomes optically thick...' can you explain the background to this sentence in more detail, or why you mention this just here in respect to Fig. 8?*
This sentence referred specifically to Fig 8. Clarified in manuscript.

L22ff: *Is there any explanation why larger particles from a more sharply peaked radiance profile?*
Clarified in manuscript: "It is characteristic of scattering by larger particles that they scatter a given wavelength more strongly. As such, in limb-viewing geometry the

radiance profile becomes more sharply-peaked as the effective particle size increases as seen in . . ."

P5327: *Again, the authors don't explain all mathematical symbols (W_{ij}; δ_{ij}). It is not possible follow the description of formula (3). Please describe this section in more detail.*
All symbols are now defined and the description of equation (3) is clarified: "In applying the MART technique each element of the cloud number density at the n^{th} iteration, $x_i^{(n)}$, is allowed to be affected by the measurement y_j by scaling $x_i^{(n)}$ by the ratio of the measurement, y_j, to its current modeled value, F_j, as . . ."

How you define/compute the measurement uncertainty for formula (3) to quantify the convergence criterion?
Convergence criteria is found empirically. Criteria is now stated in the manuscript: "Convergence toward a solution is considered satisfied when the fractional change in the state between iterations falls below 3% at all altitudes, which falls within the maximum measurement uncertainty of 10% at 750 nm (Lloyd, 2011). Typically fifteen iterations are sufficient for convergence. If convergence is not achieved, the scan is discarded for SVC retrievals."

P5328 L22: *please specify 'used in this work'.*
The phase functions from the Baum et al (2005) database. Clarified in manuscript.

P5329 section 5.3: *would the result of this section not also prove the selection of the 750nm wavelength for the measurement vector?*
The combination of two wavelengths is useful to isolate the effects of cloud properties (as you have noted), and also to provide a reliable 'background offset' reference for

the measurement vector, as described by equation 5. We have found that using this 'background' subtraction gives a measurement vector that shows better cloud sensitivity than a single wavelength in limb-viewing geometry.

Section 5.4: *It is not clearly stated if you can really retrieve surface albedo, or is it just a better estimate than a climatology for more accurate extinction retrievals? Are the albedo retrieval an useful scientific product on its own.*
This effective scene albedo is already distributed with OSIRIS data product. Clarified in manuscript.

P5330 L7: *Where are the a priori estimates based on for cloud and aerosol extinction?*
The aerosol a priori profile is from Loughman et al, 2004, for background stratospheric conditions. This has been clarified in manuscript, and a note has been added to the corresponding figure that the a priori profile is not visible on the shown scale.

L14: *The tropopause criterion is not globally valid, and will produce certain artefacts in the distribution of cloud extinctions for global analyses of retrieval results. There should be a comment on the restrictions by the simplified TP approach.*
Please see response to P5317 L7. For latitudes above 25° the thermal lapse-rate tropopause is used as a division.

L15: *The handling of the background aerosol in the retrieval seems arbitrary. Is there an objective criterion to keep the number density constant below the TP?*
Reasoning behind this constraint clarified in manuscript: "Below the Θ_{380K} altitude the aerosol number density is held fixed to a representative value of 1 cm^{-3}, and is tapered slightly at lower altitudes to indicate a transition between the Junge layer and boundary-layer aerosols. The much higher concentrations of lower-altitude aerosols are accounted for by the scene albedo retrieval described in Section 5.4."

P5332 L16: *It is not possible to judge about your statement of the uncertainties of the albedo retrieval, because these results are not presented. Again, here the paper looks more like a forward model sensitivity test or a preliminary study for the development of a new retrieval and not like a complete retrieval study.*
A study of the sensitivity of the retrieved scene albedo to aerosol loading has been done in related work. The work has been mentioned and briefly summarized: "The sensitivity of the scene albedo to aerosol loading, retrieved in this way, has been found to be at most 20% (Bourassa et al, 2012). Since the coupling of the retrieved albedo to cirrus cloud loading is similar to its relation to aerosols, it is reasonable to assume that the variability shown in this section [6.1] forms an upper bound on the sensitivity of the optical thickness to the scene albedo."

P5333 L3ff: *Is the selection of effective diameters between 40 and 60 microns really in line with the definition of SVC. You should give a reference.*
Effective sizes between 40 and $60\ \mu$m were chosen to correspond to maximum crystal dimension of between $0 < L \leq 400\ \mu$m. These effective sizes were chosen primarily to examine the effects of scattering by hexagonal plates and columns, which are dominant for these sizes. This range of effective sizes lies between the extremes noted by Lawson et al ($R_{eff} \approx 10\ \mu$m) and Heymsfield ($R_{eff} \leq 50\ \mu$m). This selection has been noted in the manuscript.

P5333 L16ff: *Following conclusions from Fig. 15 and 16 are obvious to me. Please comment on the following topics and modify/improve the corresponding section in the mansuscript: (a) there is a high sensitivity of extinction retrieval in respect to the chosen 'a priori' effective radius; (b) consequently, the quality of an operational retrieval will highly rely on the correct selection of R_{eff}, which is a very critical and difficult task, from my understanding of the presented results this seems not possible with the current approach; (c) the current retrieval process and its measurement vector* *will not deliver an unambiguous solution for the extinction retrieval due to item (b); and (d) an improved measurement vector should take into account the fitting of the measured radiances in the 700 to 800 nm region by the variation of R_{eff} (and number density instead of extinction). In summary, it feels quite unsatisfied that the results of the presented retrieval approach show that the parameter with the highest sensitivity to the target parameter and the obvious high potential to fit the spectral resolved measurements by this parameter (R_{eff}) is not taken into account. Consequently, the reader concludes that the method used the 'wrong' measurement vector (information) and asks why this result shall be published under the title 'optical property retrieval'. The presented results are very interesting for the scientific community and helpful for further studies, but appear more like a feasibility study on optical and microphysical optical properties and not like a profound study on a new parameter retrieval.*

A paragraph that clarifies the nature of this work as an optical thickness retrieval that relies on an effective size, and as a foundational study for a more profound optical property retrieval, has been included in the manuscript. This notes the requirement for spectral fitting in addition to the extinction profile retrieval. This has also been noted in the Conclusions and in the Abstract.

Tables and Figures:

Table 2: *This table can be described much more effective in text form at the corresponding section and seems dispensable.*

Table incorporated into text.

Fig. 12a: *Is the aerosol extinction really changing during the iterations (or just fixed to the a priori knowledge)? This is not detectable in the figure (but if so, then please improve the figure presentation).*

The aerosol extinctions and measurement vectors shown in this figure in the earlier version were from aerosol extinction retrievals performed *after* the cloud-extinction

retrieval, and as such showed just a small adjustment to the optically thin cloud layer. In the revised version of the manuscript the full iterations of the aerosol extinction and measurement are shown.

Fig 16: *Please improve the presentation for the three colour coded profiles, to highlight that they are more or less identical.*

A plot of the percentage difference of the calculated vectors that illustrates this fact is now included.

Anonymous Referee #2

General comments:

The Wiencz et al. article is a study on how to derive the optical properties optical thickness and particle size of thin cirrus cloud from OSIRIS limb measurements. This is shown by forward model simulations and a retrieval test case. This clearly is an interesting topic for AMT. I recommend the article to be published after the following comments are addressed.

Specific comments:

- Are aerosols in the troposphere taken into account (forward model simulations and retrieval)?

Non-sulphate aerosols in the troposphere are not taken into account. The effect of scattering aerosols and lower-tropospheric clouds are accounted for in the retrieved effective scene albedo.

- If the retrieval is very sensitive to the albedo, tropospheric aerosols must also have a large impact on the method. In the measurement vector the lower wavelengths is 470 nm (which is also used for aerosol retrievals). How does the radiance@470nm reacts when used in the FM simulations (Fig. 1a, 5, 7-9, 11)?

The 470 nm radiance profile is relatively insensitive to the presence of cloud and aerosol properties, which makes it useful as a 'baseline' component of the measurement vector. Please see response to comment P5329 section 5.3 above.

- How can lower tropospheric clouds in general be treated? Will contaminated measurements be excluded? As the lowest tangent height is at 10km, does the albedo correction catch lower clouds?

The effects of lower-tropospheric clouds are accounted for by the effective scene albedo. This has been clarified in the manuscript.

- P5333L23: *That means, the measurement vector, which is built from only two wavelengths is not enough to retrieve like optical properties like the particles and more wavelengths are needed. This must also be mentioned in the conclusions.*

A short paragraph describing this need has been added to the Conclusions section: "While it was been shown that this optical thickness is most sensitive to the a priori effective cloud particle size, it has also been shown that a spectral fitting technique using the retrieved optical thickness has potential to be used in a more complete optical property retrieval."

- Why these wavelengths were chosen in Fig.5 (550/650nm), Fig.8 and 9 (700/800nm), and Fig.11 (..)? Are they important for the retrieval or used anywhere else?

The wavelengths in Figure 5 have been changed to those used in Figures 8 and 9 (700, 750, 800 nm). A brief note has been added to explain the wavelength selection in Figure 11: "In this figure the Jacobian matrix, K, is shown for selected OSIRIS wavelengths that illustrate the change in sensitivity over the spectral range that was seen in Figure 2."

Tables and Figures:

- Fig1a: *Can the different tangent heights be colour coded and somewhat named to see which heights are there. Fig1b: Too small, the region is hard to see. Displaying only part of the image might be enough to see the region.*

The tangent altitudes have been labeled, and the in-cloud spectrum has been color-coded. Fig1b updated for clarity.

- Figures with measurement data: *an idea is to put the thick black measurement line in the background to better see the simulations.*

Figures modified in this way.

Again, please let us know if we have addressed your comments in a satisfactory way.

Warm Regards,
J. Truitt Wiensz

Interactive comment on Atmos. Meas. Tech. Discuss., 5, 5313, 2012.