**Atmospheric
Measurement
Techniques
Discussions**

Interactive
Comment

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

Anonymous Referee #1

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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

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been performed.

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).

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.

Specific comments:

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

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model and the retrieval quantities.

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.

P5315: Generally, acronyms have to be explained when mentioned first, here e.g. SAGE II, CALIPSO, CPI, Odin.

p5316: 'modelling thin cirrus observations' please specify: optically and/or vertically thin?

P5317 L7: Is the Θ=380K an objective criterion for the tropopause, please specify and/or give a reference.

P5317 L17: What's 'odin', please clarify the acronym.

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?

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)?

P5319 L4: Are there any polarisation effects in the measurements?

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

L11ff: Please explain all formula symbols ω, P. It seams to me this paper is missing in some parts a carefully proof-reading.

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'?).

P5321 L25: Please specify optically or vertically thin.

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).

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.

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.

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.

P5324 L6: How you estimate the cloud thickness of 300m, from the measurements? Please specify.

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.

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.

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?

L22ff: Is there any explanation why larger particles from a more sharply peaked radiance profile?

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.
How you define/compute the measurement uncertainty for formula (3) to quantify the convergence criterion?

P5328 L22: please specify 'used in this work'.

P5329 section 5.3: would the result of this section not also prove the selection of the 750nm wavelength for the measurement vector?

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.

P5330 L7: Where are the a priori estimates based on for cloud and aerosol extinction?

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.

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?

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.

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.

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

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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.

Tables and Figures:

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

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).

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

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