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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Optical extinction by upper tropospheric/stratospheric aerosols and clouds: GOMOS observations for the period 2002–2008" *by* F. Vanhellemont et al.

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We will reply to the comments point by point. Note: Original comments of the referee are given in italic for clarity.

General Comments: The paper by presents the analysis of nearly 7 year of GOMOS optical extinction data. A long record of extinction data with good global coverage and moderate vertical resolution is now available. The paper is well written and structured. The special section on GOMOS results in ACP seems exactly the right place for publication.

Response: We thank the referee for his/her time and energy to review our paper and

suggest useful corrections.

The weakness of the paper is that the authors didn't highlighted what is actually new in this publication compared to the Vanhellemont et al. (2005) article. Is it just the same analysis now for seven years instead of only one year, or is this paper not the presentation of a real climatology compared to Vanhellemont et al. (2005).

Response: Agreed. This is something we can better do at the end of section 1 (Introduction), where we already mentioned the previous paper from 2005. We changed the paragraph as follows: "First results on GOMOS aerosol/cloud extinction profiles representing the year 2003 were previously published (Vanhellemont, 2005). The data discussed here span a much longer time period, from 2002 to 2008. Furthermore, a new data version was used, with as most important feature the use of a quadratic polynomial of wavelength as aerosol extinction model, while the previous model as described in (Vanhellemont, 2005) was oversimplified and consisted of a fixed inverse wavelength function."

The authors should emphasise in more detail on the strengths and objectives one can achieve with this new climatology-like dataset, for example advantages compared to other instruments, probably the better sensitivity, the compilation of a continuing dataset in respect to SAGE and POAM instruments, and especially the excellent detection of weak volcano signals. I recommend to do this to some extend already in the abstract.

Response: We partially agree. We wouldn't say that GOMOS has better sensitivity with respect to other instruments (it is a star occultation instrument, the S/N ratio is much lower than solar occultation instruments such as SAGE and POAM). Also the excellent detection of weak volcano signals was already mentioned in the abstract. However, the continuing of data sets from discontinued instruments such as SAGE and POAM is however a good point, and other advantages are the large number of occultations and the near-global coverage.

We included these issues at the end of the abstract as follows: "Therefore, the importance of the GOMOS aerosol/cloud extinction profile data set is clear: a long-term data record of PSCs, subvisual cirrus, and background and volcanic aerosols in the UTLS region, consisting of hundreds of thousands of altitude profiles with near-global coverage, with the potential to fill the aerosol/cloud extinction data gap left behind after the discontinuation of occultation instruments such as SAGE II, SAGE III and POAM III."

Specific comments:

Abstract:

The text is not highlighting what kind of results are really new, for example the authors might give some examples for the opportunity of new analyses.

Response: We are sorry, but here we do not fully understand the comment by the referee, specifically the opportunity for new analyses. Is he referring to analyses that can be performed in the future? If so, what does this have to do with the results that are new in this paper?

"can be expected" sounds a bit vague for an abstract, where you have already confirmed the good quality of the data in the paper.

Response: Agreed. We have changed "can be expected" by "are obtained".

Please skip the last sentence and summarise a few results.

Response: Agreed. We removed the last sentence of the abstract and added: "For the first time, we show comparisons of GOMOS 500 nm particle extinction profiles with the ones of other satellite occultation instruments (SAGE II, SAGE III and POAM III), of which the good agreement lends credibility to the GOMOS data set. Yearly zonal statistics are presented for the entire period considered. Time series furthermore convincingly show an important new finding: the sensitivity of GOMOS to the sulfate input by moderate volcanic eruptions such as Manam (2005) and Soufriere Hills (2006). Finally, PSCs are well observed by GOMOS and a first qualitative analysis of the data

agrees well with the theoretical PSC formation temperature. Therefore, the importance of the GOMOS aerosol/cloud extinction profile data set is clear: a long-term data record of PSCs, subvisual cirrus, and background and volcanic aerosols in the UTLS region, consisting of hundreds of thousands of altitude profiles with near-global coverage, with the potential to fill the aerosol/cloud extinction data gap left behind after the discontinuation of occultation instruments such as SAGE II, SAGE III and POAM III."

Introduction: The introduction summarises very nicely the current status on stratospheric aerosols, but I would suggest a few more references, for example in section 2 the SPARC report on Stratospheric Aerosol and at the end of section 3 a review paper on PSC formation.

Response: Agreed. In the paragraph on stratospheric aerosols we added: "A good overview of stratospheric aerosol science can be found in (SPARC, 2006)". At the end of the paragraph on PSCs we added: "A more detailed description of PSC formation can be found in the review paper of Zondlo et al. (2000)".

Retrieval Method: I am missing the details on some instrument parameter like the field of view and vertical sampling.

Response: Agreed. We added the following phrase to section 2 (GOMOS: instrument and obtained data set) : "The integration time to record a GOMOS full spectrum is 0.5 seconds. The actual vertical sampling is determined by this time, together with the vertical velocity of the tangent point (between 0.5 and 3.4 km/s, depending on the obliquity of the occultation) and refraction of the optical path at lower altitudes (which decreases the tangent point vertical velocity). A maximum vertical sampling resolution of 1.7 km can be expected, but during very oblique occultations a 200 meter sampling is obtained. The actual star spectrum is (in normal mode) calculated from 7 CCD rows of the spectrometers, equivalent to a field of view of 0.01 degrees."

There isn't a quantitative discussion on retrieval errors. A number of problems and restrictions are mentioned but the resulting error in extinction is not presented. The

smoothing effect is obvious, but the presentation of averaging kernels would allow to quantify the vertical resolution. Please add some more quantitative error discussion.

Response: We partially agree. A detailed discussion of the retrieval errors and averaging kernels has been given in another paper of this special issue ("GOMOS data characterization and error estimation", Tamminen et al, ACPD 10, 6755-6796, 2010) and a full discussion of these matters seems unnecessary to us. However, we have repeated the main results for aerosols from the paper by Tamminen in our paper here.

We have added the following paragraph at the end of subsection 3.1: "Quantifying the aerosol extinction retrieval error is challenging. A detailed description can be found in another paper of this GOMOS special issue (Tamminen et al., 2010). We repeat the most important ideas and findings here. The random error on a profile is determined by two contributions that we mentioned before: (1) the measurement noise which changes from one stellar source to another due to star magnitude and temperature differences, and (2) the uncorrected residual scintillation component. At the time of writing the GOMOS error estimation for the operational data products does not yet take the latter into account, so that retrieval errors are likely underestimated. The influence of star magnitude is clear: brighter stars deliver a better signal-to-noise ratio. Star temperature determines the main spectral emission range: hot stars emit in the UV, colder ones in the visible and near-infrared domain. The influence of star temperature on aerosol retrievals nevertheless remains limited; it is star magnitude that plays the crucial role (Tamminen, 2010). Sources of systematic error are of course (1) a possibly wrong aerosol spectral model, and (2) an imperfect ECMWF air density profile, both of which have been estimated by Tamminen et al. (2010). Retrieval errors are of course calculated by standard error propagation through the retrieval chain. Aerosol extinction error estimates (for bright stars) of 30

"As mentioned before, the amount of Tikhonov altitude smoothing is determined by a predefined target resolution of 4 km, at all altitudes, regardless of the star magnitude and temperature. Presentation of averaging kernels is therefore unnecessary; the

profile resolution is chosen in advance".

Comparisons: Why do the authors exclude PSC from the analyses but cirrus are taken into account. I guess PSC are easier to exclude. However, the following arguments in the manuscript (P11118L11-15) are also applicable for (subvisible) cirrus clouds around the polar tropopause. Please clarify.

Response: PSCs are typically irregularly shaped, with the result that two different observation geometries lead to large differences. This is less the case for cirrus clouds, for which it is known that they are horizontally extended cloud layers that are very thin; the viewing geometry has a smaller effect here, which means that we can use the measurements for comparisons. We added the following sentence to the paragraph: "This is less of a problem for tropical cirrus since they typically have a very wide horizontal extent and are very thin."

Results: There should be a comment why and how you handle negative (physically meaningless) extinctions in the retrieval and the analysis (Fig. 6).

Response: Agreed. This is something that we forgot to mention. Basically, the final retrieval step (spatial inversion) is a linear least-squares problem, and therefore the unknowns (gas concentrations, aerosol extinction coefficients) can assume negative values. No effort is taken during spatial inversion to constrain the retrievals to be positive. In situations where the retrieval error is large, the profiles can become negative. It is important to keep these negative values during further data analysis. One negative value is perhaps physically meaningless, but the average of a large ensemble will be positive. Cutting negative values before taking the average would result in bias.

We added a paragraph to section 3.5 (Retrieval results) since the discussion is important for all following sections (comparisons, yearly zonal means, etc.): "Finally, we should mention here that the obtained particle extinction retrievals sometimes assume negative values, usually at altitudes where the measured signals are low (below the above-mentioned cut-off altitude), or where the particle abundance is low (upper strato-

sphere and higher). This is a logical consequence of the fact that the last retrieval step (spatial inversion) is linear and that the retrievals are not constrained to be positive. All further results that are discussed in this paper were obtained by processing of data that include negative values; discarding these would lead to biased results."

To my mind it is more precise to say that GOMOS 'detects' PSC quite well, because so far the type classification is not possible.

Response: Agreed. We have changed the word "observes" to "detects".

The PSC temperature analysis is only a very rough approach, the results are looking very noisy and are not convincing me. The are missing informations: Is the temperature a retrieved quantity or based on meteorological analyses?

Response: Agreed, we should have been more clear here. The temperature is not retrieved, they are ECMWF analysis profiles. We added the following sentence: "Temperature data were obtained from the GOMOS product files and consist of ECMWF analysis profiles".

The PSC formation temperature depends on H2O and HNO3 as well. Is this taken into account?

Response: No, not at all. This lies really outside the scope of this paper. Our goal was to give an overview of the GOMOS aerosol extinction data set for the GOMOS special issue, highlighting some observations of interest (such as the temperature dependence of particle extinction), in order to show the potential of the data. This paper is not about detailed scientific modeling of PSCs, which is a study on its own (probably to be done in the future).

The radius dependence is also not very obvious in the presented data, maybe there is a better way of visualise the information content of Figure 9b (e.g. 2D probability density).

Response: Actually, the entire point of the figure was to show that the radius depen-

dence is very noisy, due to the unfortunate implementation of the aerosol spectral law in the retrieval processor (which is mentioned in the text). This situation will be improved in the near future with a better algorithm, but the figure represents the situation as it is now: the radius dependence IS not obvious. We therefore prefer to leave the figure unchanged.

Please specify and highlight the limits of this qualitative analysis of section 5.3. Consequently (so far) the analysis allows to draw only 'moderate' conclusions (see below).

Response: Agreed. We added the following sentence at the end of the section: "The presented PSC results do not add significantly new information to the current knowledge on PSC formation; for this, much more detailed analysis is needed (taking into account all thermodynamic parameters and air parcel dynamics). But the GOMOS measurements clearly contain elements (temperature dependence of extinction and particle sizes) that are crucial in such a detailed study".

Conclusions: The presented PSC analysis was definitively not sufficient to 'clearly confirm' the theoretical PSC temperature dependence. Please change the wording in the sense of the qualitative and not very detailed analysis.

Response: Agreed. We have removed the sentence "Here, the theoretical PSC temperature dependence was clearly confirmed by GOMOS observations". We have added: "The dependence of PSC formation on temperature is a complex study topic. In this paper, we only showed a first qualitative analysis with GOMOS data, the results of which nevertheless agree with the theoretical PSC temperature dependence."

Technical corrections:

P11114, L23: please explain the acronyms for extinction and wave length.

Response:Agreed. We have changed "($\beta \sim \lambda^{-4}$)" to "(the Rayleigh limit: extinction β depends on wavelength λ as $\beta \sim \lambda^{-4}$)".

P11118, L15: Is the coincidence window +/- 12 hours? This would be a quite a large

value. The number of coincidences would help to visualise the statistically significance of the analysis.

Response: No, a +/- 6 hours window was used. The number of coincidences have been summarized in Table 1, and the large number of coincidences shows that the statistical significance is quite good. To avoid confusion we have changed the text "A coincidence window of 500 km and $\frac{1}{2}$ day" to "A coincidence window of +/- 250 km and +/- 6 hours".

P11125, L9: typo - "and GOMOS is"

Response: Agreed. We have corrected the typo.

Fig. 6: It might help to visualise the jumps in the mean extinction by adding a 50

Response: We did not understand the suggestion of the referee because the sentence does not make sense. Probably something went wrong during the generation of the referee comments digital file.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 11109, 2010.