Atmos. Chem. Phys. Discuss., 10, C6514–C6521, 2010
www.atmos-chem-phys-discuss.net/10/C6514/2010/

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.

F. Vanhellemont et al.

filip.vanhellemont@aeronomie.be

Received and published: 16 August 2010

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

General Comments

This paper is well written and does a nice job of highlighting pertinent features in the GOMOS aerosol record. This special section of ACP is an appropriate place for publication.

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

suggest useful corrections.

As mentioned by the other referee, I would recommend that the premise of the paper be somewhat revised to focus on the new results that are presented, in particular the comparisons with SAGE II/III and POAM III, and the identification of perturbations from lower stratospheric eruptions in the time series.

Response: Agreed. We emphasized the new results on comparisons and the detected volcanic eruptions, by changing a few sentences in the abstract and the conclusions.

In the abstract, the sentence on comparisons was changed to: "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." The sentence on volcanic eruptions was changed as follows: "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)."

In the conclusion, we adapted the sentence on comparisons as follows: "For the first time, a comparison with SAGE II, SAGE III and POAM III was presented, showing good agreement within 20 % in the upper troposphere/lower stratosphere, from 10 km to about 25 km." A sentence on volcanic aerosols was also changed: "Here , for the first time, the aerosol enhancements resulting from the eruptions of Manam (Papua New Guinea) and Soufriere Hills (Montserrat, West Indies) have been identified in the GOMOS data."

Specific Comments

Several difficulties in the retrieval of aerosol extinction profiles are well explained in the paper; however, some aspects could be addressed in more detail to give the reader some needed insight into how these difficulties impact the results. For example, the *apparent trade-off between vertical smoothing and residual scintillation perturbations is clear, yet the choice of the 4 km smoothing criteria is not quantified. What is the magnitude of the scintillation error without smoothing, and is the residual reduced to a completely negligible quantity by the smoothing?*

Response: The 4 km target resolution was actually chosen for the retrievals of gases (NO2, NO3, O2 and H2O) as well as aerosols. The magnitude of the residual scintillation perturbation on gases and aerosols has at the time of writing only been quantified for ozone (see the associated paper by Sofieva et al in this special issue). The choice of 4 km has mainly been chosen because it gave acceptable results for the gases. For aerosols/clouds we now feel that 4 km is a quite severe (as we already mentioned in the paper) since actual features such as thin clouds are also smoothed. Furthermore it is not certain whether or not the perturbation is reduced to a negligible quantity. All we can say is that first comparisons with other instruments do not indicate systematic errors at altitudes where the scintillation occurs.

For clarity, we added the following text to section 3.2 (spectral/spatial inversion): "The choice of 4 km for target resolution was based on the experience that it gave agreeable results; the actual magnitude of the residual scintillation perturbation on aerosol profiles is still not adequately determined, although it is known for the ozone retrievals (Sofieva et al., 2009). "

Also, we added the following statement to section 3.5 (retrieval results), as was also demanded by referee 1: "There is reason to assume that the scintillation perturbations do not introduce bias in the extinction profiles, as comparisons with other instruments show (see below)."

Similarly, the noise at "other" wavelengths (what are they, by the way?) can certainly be understood by the lack of constraint in the spectral fit, but could this be quantified?

Response: With "other wavelengths" we mean every possible wavelength of the spectral range that is used for the spectral fit, which means spectrometer A (248-690 nm).

Given the fit parameters of the quadratic polynomial, the extinction can be evaluated at every desired wavelength, including the associated (co)variances. So the errors are easily quantified at other wavelengths. However, at this stage, and within the context of this paper, we don't see the point of doing this: we know that the extinction is extremely noisy outside 500 nm, and therefore we present only 500 nm results in the paper, the other wavelengths are discarded. The other wavelengths will be analyzed and discussed in the future with an improved data set.

To make the text more clear concerning the other wavelengths, we changed the text in section 3.2 (Spectral/spatial inversion) as follows: "... the spectra at other wavelengths (evaluated with the quadratic polynomial within the spectrometer A range 248-690 nm) are often very noisy."

Are errors derived as part of the retrieval and if so could they be shown on the profiles in Fig 2 and compared to the percentiles shown in the comparisons of Fig 3?

Response: Yes, they are part of the GOMOS L2 data product files. We have included the errors in a new version of the figure. However they can not be directly compared with the percentiles of Fig. 3: the error bars are the retrieval errors of profiles that were severely smoothed with Tikhonov regularization, and they are therefore quite small. Much smaller than the statistical spread that can be seen on Fig. 3. This is of course because the error bars do not contain the so-called 'smoothing error' (see Rodgers theory), a component that is difficult to estimate. In the near future, a new aerosol extinction product will be available that receives much less Tikhonov smoothing, and for which the error bars will be more realistic.

Could you please comment on the significance of star brightness and temperature for the 500 nm aerosol extinction retrieval?

Response: Agreed. We have written some new material on retrieval error estimation (also in response to referee 1), in which we describe the effect of star magnitude and temperature on the retrieval:

" ... 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) "

In section 3.5, the cut-off altitude for 500 nm is discussed; does is not also depend on optical depth, i.e. volcanic plume or cloud in the line of sight? Again a quantification of the error would be helpful in understanding the choice of cut-off altitude.

Response: Of course. A measured signal can be considered as lost when the signal drops below the noise level. Where exactly this happens depends on star characteristics, and the considered wavelength, as was mentioned in the paper. But it also depends on the amount of light extinction along the optical path, a fact so obvious that we forgot to mention it. We therefore adjusted a sentence of section 3.5: " ... are increasingly more uncertain when we descend to lower altitudes, because the transmitted light intensity becomes weaker due to increasing atmospheric extinction by gases, aerosols and clouds."

The cut-off altitude that we mentioned (10 km) is not a quantity that was accurately determined with the use of some prior condition; it was only a very rough estimate, since the individual cut-off altitude for each profile varies from maybe 5 km, up to 15 and sometimes even 20 km for extremely weak stars. We changed the text in a subtle way: "At 500 nm, an average limit of 10 km can be considered as a rough estimate below which the profiles are not trustworthy anymore."

Why is the extinction negative below the volcanic plume shown in Fig 2? Is this within the error bar?

Response: the GOMOS gas concentration and extinction retrievals can become negative since no positivity constraint is applied: the last step is a simple linear inversion. We added some explanation in Section 3.5 (Retrieval results): "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 stratosphere 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."

As can be seen on the new Fig. 2, error bars are larger than 100 % where the extinction is negative, so the negative points are statistically insignificant.

A statement is made regarding the "disappearance" of the residual scintillation in the zonal mean. I understand what the authors are alluding to, but this sentence should be reworded carefully as a smooth mean does not require that the perturbations have zero mean.

Response: Agreed. It is not certain that the perturbations have zero mean (although intuitively it should be so). This means that the averaged profile is smooth, but perhaps contains a bias. We changed the sentence (Section 3.5, first paragraph) to: "When averaging large numbers of profiles (see e.g. the zonal plots below), the resulting mean profile becomes smooth. There is reason to assume that the scintillation perturbations do not introduce bias in the extinction profiles, as comparisons with other instruments show (see below)."

The comparison with SAGE II/III and POAM III is very useful. However, more details on the data set would be helpful. For example, how many profiles are compared in each case and what is the geographic distribution of the coincidences?

Response: We partially agree. The number of coincidences have already been given in Table 1. The geographical distribution of the coincidences is mainly determined by the geographical coverage of the other instruments (SAGE II, SAGE III, POAM III), since the coverage of GOMOS is near-global and since many latitudes (typically 30) are sampled during even one orbit (because stars are used as light source). Without going into too many details (since this is not a full-blown validation study), we added the following sentence: "The geographic location of the obtained coincidences is mainly determined by the coverage of the used instruments (SAGE II, SAGE III and POAM III) since GOMOS has a near-global coverage with several occultation latitudes per orbit."

Is any effort made to match the vertical resolutions of the measurements for the comparison?

Response: No. We really think that it is too soon to go into the real details of a full validation study, since the data products are still far from perfect. The comparison with SAGE II/III and POAM was merely to show (within the context of the GOMOS special issue) that the GOMOS aerosol profiles make sense. Matching vertical resolutions of course will be necessary for the validation of the upcoming improved aerosol data set.

For clarification, we added the following sentence to section 4 (Comparisons with other instruments): "We should also mention that no effort was made to match the vertical resolution between two instruments with averaging kernels; this will certainly be done in a full validation study in the future."

The statements made regarding the detection of stratospheric features with CALIPSO/CALIOP should be revised in the light of the recent publication of Vernier et al., JGR, 2009. In fact, some discussion of these results would be pertinent to the volcanic aerosol section.

Response: Agreed. Actually, this publication escaped our attention since it was published almost simultaneously as the submission of our paper. In any case, the paper by Vernier et al shows (1) that the CALIOP data have been improved significantly thanks

to a new calibration and cloud mask, and (2) that the Soufriere Hills eruption was indeed replenished with sulfate aerosols from the Tavurvur eruption several months later. So we changed our text. In section 5.2 (Volcanic aerosols) we added the following sentence to the paragraph on CALIOP versus occultation measurements: "Nevertheless, very recently Vernier et al. (2009) showed how CALIPSO/CALIOP backscatter data improved strongly after the introduction of a new cloud mask and a new calibration, and how an improved stratospheric aerosol picture emerged after sufficient data averaging." We also added the following sentence to the last paragraph of section 5.2: "The latter case for Tavurvur was recently shown in a convincing way using the improved CALIPSO/CALIOP data by Vernier et al. (2009)."

Fig 6: I cannot see the first vertical line, and would it be possible to show the entire data set rather than the subset of years that are shown?

Response: We partially agree. The first vertical line was added. However, we tried to make the time series longer, but then the 'tail' of the Manam volcano wasn't clear anymore (it's already difficult to see due to the data gap). The entire period from Jan. 2005 to May 2006 became compressed and it's difficult to see the decay. So we prefer to leave the figure as it is. Besides, Fig. 7 already shows the same time series for a longer period.

Could the data set be extended into 2009 to show the effects of the relatively large eruptions of Mt Kasatochi (July 2008) and Sarychev Peak (June 2009)?

Response: Of course, but at the time of writing, we only had access to data until May 2008. Updating the data time series will have to wait until a next data version. By that time the quality of the aerosol profiles will have been improved considerably.

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