Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2016-28-AC2, 2016

Atmospheric Measurement Techniques Discussions
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EGU

Interactive comment on "AerGOM, an improved algorithm for stratospheric aerosol extinction retrieval from GOMOS observations. Part 1: Algorithm development" *by* Filip Vanhellemont et al.

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Received and published: 1 June 2016

Comments on the review by Anonymous Referee #2 of the paper: "AerGOM, an improved algorithm for stratospheric aerosol extinction retrieval from GOMOS observations. Part 1: Algorithm development." by Vanhellemont et al.

We thank referee#2 for the comments on our article.

1. Overall Comments

1.1

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Discussion paper

Referee #2:

The paper is poorly written. One gets the impression that a project report has been hastily converted into a scientific paper. The material is poorly organized and the sentences are poorly structured.

Author response:

These comments came as a complete surprise to us. Before sending the draft to AMTD, it was proofread by 11 well-respected scientists, and none of them found it to be poorly written, quite the contrary. We would like to point out that we take the publication process very serious; before submission, a draft paper is evaluated in detail, corrected (scientific content, style, grammar, syntax), and rewritten until we are confident about the quality. Hence our surprise. The referee has the impression that the paper was hastily converted from a project report. We can assure the referee that this is only an impression; although parts of the paper are based on a number of ESA project reports, the text itself has been written from scratch. The referee thinks the material is poorly organized, but does not specify what constitutes a good paper organization according to him/her. Finally, the referee finds the sentences poorly structured, but once again does not specify which sentences (see for contrast the detailed comments by Referee#1). It is impossible to defend ourselves against criticism that is not specific, or to improve work if it is not specified where exactly things are not satisfactory. We are not talking about the more specific comments below, only about this first comment.

1.2

Referee #2:

It seems most of the changes described in the paper constitute routine clean up of a computer code rather than significant scientific advance. Most of the material can be deleted, put in an appendix, or provided as a supplement to the paper.

Author response:

In our minds, routine cleanup of a code consists of updating physical data (constants, absorption cross sections etc.) and physical laws, optimizing processing speed etc. Apart from the changes concerning air refractive index (Section 3.2) and the Rayleigh cross section (Section 3.3) which can indeed be considered as updates, the rest of the AerGOM code differs considerably from the GOMOS IPFv6.01 processor. The methodology and the consequential architecture of the algorithm is completely different (accounting of all covariances between species, spatial inversion of all species simultaneously). It is most certainly not a simple update of the GOMOS IPF code: 'transforming' IPFv6.01 into AerGOM would only be possible if very large parts of the IPF code were changed beyond recognition.

1.3

Referee #2:

I do not think that there is enough material in this paper for it to be a separate paper. My suggestion is to combine this paper with the proposed follow-on paper that discusses results.

Author response:

Obviously we differ in opinion, otherwise we would have thought about submitting only one paper ourselves. To clarify our decision: The first AerGOM paper (on the algorithm) would presently be 13 pages long (in final print), the second paper (comparisons) 15 pages. This totals to 28 pages for one unified paper. Even if we reduce the length of the first paper by 50 %, a total length of 21 or 22 pages would be obtained, which is too long in our opinion. To give some idea: a quick look at the AMT website shows that most of the accepted papers have a length of about 10-13 pages. But there is another argument in favour of two separate papers: part of the present and future AerGOM data users are not interested in retrieval details but only want to know how good the data are; these readers only have to consult the second paper. On the other hand, retrieval code developers have a major interest in these details, so these should be

published as well. In the end, a split paper is the most logic choice to us.

2. Detailed Comments

2.1

Referee #2:

The entire text in written in passive voice. Rewriting the sentences in active tense will make the paper lot easier to read.

Author response:

We did a quick count of all active and passive phrases on the first 6 pages of the draft paper, and arrived at more active than passive phrases. So the statement that the entire text is written in passive voice is not correct. Furthermore, there is no consensus on the use of active versus passive voice in scientific papers, as can be easily deduced from the large amounts of diverging opinions (published in journals) on the internet. Finally, passive voice is used a lot in papers accepted by AMT (we encourage the referee to have a look on the AMT website). Although we respect the referee's personal preference, we don't think it's a valid objective criterium to evaluate a paper in a peer review. Perhaps it will be in the future, when a consensus is reached.

2.2

Referee #2:

In the title change "development" to 'description".

Author response:

Agreed. The word 'description' better suits the paper content.

Author's changes in manuscript:

The title has been changed according to the referee's suggestion.

2.3

Referee #2:

The abstract doesn't provide much information other than to say that previous algorithm had some problems, some of those problems have been fixed. Abstract should provide key results rather than just an outline of the paper.

Author response:

Agreed. We have shortened the part of the abstract that outlines the paper, and instead provided some more details on the results. However, since the detailed validation of the data is covered by the accompanying paper, the discussion on results is deliberately kept brief.

Author's changes in manuscript:

We have removed the last part of the abstract that outlines the paper (lines 7 – 11), and replaced it with the following text: 'The AerGOM algorithm differs from the standard GOMOS IPF processor in a number of important ways: more accurate physical laws have been implemented, all retrieval-related covariances are taken into account, and the aerosol extinction spectral model is strongly improved. Retrieval examples demonstrate that the previously observed profile perturbations have disappeared, and the obtained extinction spectra look in general more consistent. We present a detailed validation study in a accompanying paper; here, to give a first idea of the data quality, a worst-case comparison at 386 nm shows SAGE II /AerGOM correlation coefficients that are up to one order of magnitude larger than the ones obtained with the GOMOS IPFv6.01 data set.'

2.4

Referee #2:

Introduction is verbose, repetitive, and too self-congratulatory. I do not know what is

meant by, “The GOMOS instrument has de facto become the reference spaceborne stellar occultation instrument”, since not many stellar occultation instruments have flown in space.

Author response:

(1) On the ’verbose’ comment: we do like to construct full sentences in order to make ourselves clear. Some readers might think that we use too many words; others (especially those that have only a limited background in the field) will appreciate it. Apart from this, it really is a matter of taste. We are open to criticism however, especially if it improves the paper quality. But once again: referee#1 does not specify what should be removed. How are we to know this? The reason we sent the text in its current form is exactly because we are satisfied with it. If the referee wants the introduction to be shortened, he/she should specify which material should be removed. (2) On the ’repetitive’ comment. We tried very hard to find a statement or piece of information that is given more than once in the introduction, but we couldn’t find any. Also here, we do not know what to do with the referee’s comment. It is not specified where the text is repetitive (and we doubt it is). (3) Regarding the comment on the ’self-congratulatory’ introduction and the ’de facto’ sentence: the expression ’de facto’ refers to a situation that has come into existence simply by fact. The sentence in the paper simply states that GOMOS has become the reference instrument because it is the only stellar occultation instrument. That’s what ’de facto’ means. Referee#2 interpreted this wrongly, since he/she comments: ’...since not many stellar occultation instruments have flown in space’ (which is exactly what we wanted to say). Furthermore, the referee finds all this self-congratulatory. We really don’t understand. Perhaps ’de facto’ has some connotation of which we are not aware (none of the authors are native English speakers). All of this is very unclear to us. To simply avoid the discussion, we have decided to remove the phrase.

Author’s changes in manuscript:

We have removed the 'de facto' part. The new sentence reads: 'The GOMOS instrument and its 10-year quasi-continuous operation can be considered a success'.

2.5

Referee #2:

There is far too much detail in section 2. If the paper has already been published why not provide just short summary that is relevant for understanding section 3.

Author response:

The material in section 2 is already a short summary. We want to stress that we have given only the bare minimum of information on GOMOS and its IPF processor. For comparison: the cited papers by Kyrola et al (2010) and Bertaux et al (2010) are respectively 23 and 58 pages long. That's a lot of material. We have taken only the information that is of direct relevance to our paper (more specifically section 3) and condensed it in a 2-page summary. Without this minimum information, our paper would be difficult to understand. Furthermore, we want to explain in the paper the reasons why the AerGOM algorithm constitutes an improvement with respect to IPFv6.01. This is only possible if we give a summary of IPFv6.01 first. In our opinion, referencing other papers is good when specific details are necessary, but in general the reader should be able to understand the text without constantly having to consult other publications.

2.6

Referee #2:

Section 3.1: It is best not to bulletize the text in a scientific paper. They could be shortened and combined into a paragraph.

Author response: Agreed.

Author's changes in manuscript:

Bullets have been removed, the text is shortened and now appears as a paragraph. Furthermore, since bulletized text is also present in section 3.1 of the draft paper, we removed the bullets there as well and have rewritten the text into a paragraph.

2.7

Referee #2:

In what way these changes are “fundamental”. To me they appear to be fairly routine clean up of a code in which several approximations were made that are now being improved.

Author response:

See our comment above on ’routine clean up’ versus change in retrieval methodology and code architecture. Whether or not this is fundamental or not is (in our opinion) not that important. We have removed the word.

Author’s changes in manuscript:

page 7, line 5: ’There are however several significant differences:’.

2.8

Referee #2:

Section 3.2: I have no idea whether this constitutes significant improvement or it is just a minor issue.

Author response:

Actually the impact of the new refractive index law (Peck and Reeder) is quite small. The impact on the Rayleigh cross section is well below the 1% level. We implemented the Peck & Reeder law because it is slightly more accurate than the Edlen law (used by the standard GOMOS algorithm) , which is a legitimate reason to use it. But the AerGOM data improvements are not caused by it. With this in mind, we admit that the

topic receives too much attention in the paper.

Author's changes in manuscript:

We have removed subsection 3.2 ('Refractive index of air'). Instead we have added a sentence to subsection 3.3 ('Rayleigh scattering by the neutral density (air)'): 'The air refractive index mstp is evaluated using the equation of Peck and Reeder (1972), that is slightly more accurate than the still widely used Edlen law (Edlen, 1966) (also used in the GOMOS IPF processor).'

2.9

Referee #2:

Section 3.4: The text is verbose. Most of what is in there could be said in fewer words.

Author response:

See our reply to the comment on verbosity above. The referee does not specify what should be removed. It is impossible to change the text without knowing this.

2.10

Referee #2:

Fig 3 is interesting but it would be useful to provide details of the models that were used in deriving these spectra in a tabular form, including altitude, refractive indices and size distributions.

Author response:

Agreed.

Author's changes in manuscript:

Two tables have been added: one with size distribution parameters for the remote sensing data, and one with in situ size distribution data (impactor data). Altitudes and

refractive indices are also given. We also added numbers to Fig. 3 that refer to the data in these tables. Finally, an extra paragraph was added at the end of section 3.3.3 that describes these tables, the data sources and the method to derive the extinction spectra: 'As an example, we used the SAGE II/CLAES stratospheric aerosol climatology of Bauman et al. (2003), and converted a few of their values for effective radius Reff, mode width and 525 nm aerosol extinction (respectively Figs. 4, 8 and 11 in the paper) to the values in Table 3. Furthermore, stratospheric in situ data derived from impactor samples collected onboard an ER-2 aircraft (Pueschel et al., 1994), were used (see Table 4). In both cases, we assumed US76 temperatures at the considered altitudes, and derived corresponding H2SO4 weight percentages with the method of Carslaw et al. (1997). Refractive indices were obtained with the method of Krieger et al. (2000). Finally, we calculated the extinction spectra in Fig. 3 with a Mie code. Also shown are the fits with the quadratic polynomial of inverse wavelength; the correspondence is quite good.'

2.11

Referee #2:

Section 3.8 is too brief. It is very hard to conclude anything from it.

Author response:

The referee does not specify what needs to be added, nor does he/she mentions which subsection should be changed. We assume that the referee is not talking about subsection 3.8.1 ('AerGOM processing'), only about 3.8.2 ('A first look at the AerGOM results'). As the title suggests, and the text mentions, the purpose is not to present detailed validation results; this is the subject of the accompanying paper. We only wanted to show visually that there is lots of improvement. So the only conclusion that can be drawn from this paper is: the code works, and delivers improved data (the profile perturbations are not there anymore, the spectra look better). Exactly how good are the data? This is answered in the second paper. However, to give a first quan-

titative idea on the data improvement, we have added correlation coefficients to the SAGEII/AerGOM comparisons in Fig. 7, clearly showing the improvement (up to one order of magnitude at high altitude).

Author's changes in manuscript:

We have added the correlation coefficients in the title of every subplot (Figure 7), and a sentence in the Figure caption: "Correlation coefficients are also indicated in the subplot titles". Furthermore, we have added the following discussion on the correlation coefficients: "This is confirmed when we inspect the correlation coefficients (also given in Fig. 7). Although relative aerosol extinction contributions at the considered wavelength of 386 nm are weak, correlation coefficients are significantly larger for the SAGE II/AERGOM case, even up to one order of magnitude at 29 km."

2.12

Referee #2:

Results shown in Fig 6 look strange. I find it difficult to believe that spectral dependence of aerosol extinction in the stratosphere has such large variability, particularly at 30 km. Aerosols at these altitudes mainly consist of fine mode particles. So I would have expected the log of aerosol extinction to be more or less linear with log of wavelength. It seems that the quality of data is poor below 500 nm, which is not surprising given rapidly decreasing ratio of aerosol/molecular extinction.

Author response:

The answer is already present in the comment from the referee. At short wavelengths, the aerosol/molecular extinction ratio is small. This results in larger aerosol extinction retrieval errors. These increased error bars are not shown in Fig. 6, but can be deduced from the divergence of the extinction values below roughly 400 nm. Of course, this means that the observed curvature of the spectra (typically below 350 nm) is statistically insignificant. Basically, the aerosol information content is so low that the

assumed aerosol spectral law (quadratic polynomial of inverse wavelength) is free to fit whatever it wants. Summarized: at high altitudes, where indeed only small particles are expected, the observed deviation from a power law spectrum at short wavelengths is not significant since the error bars on aerosol extinction are very large. We might as well not show the data below 350 nm but we choose to do it anyway, since the GOMOS transmission data at these wavelengths have been used in the retrieval process. There really is no ambiguity; the AerGOM v1.0 data set contains aerosol extinction retrieval errors, and the user will certainly notice the increased error bars at short wavelengths. We also need to comment on the reduced quality of the data below 500 nm. This is true, but it doesn't mean that aerosol information is completely absent. See for example the SAGE II/AerGOM comparisons in Fig. 7 at 386 nm. Besides, without aerosol sensitivity, SAGEII aerosol extinction at 386 nm wouldn't even be provided in the SAGE II data files.

Author's changes in manuscript:

For clarification, we have added the following text (page 15, line 15): 'Notice the increased variability of extinction values at short wavelengths (below 400 nm), reflecting the larger retrieval errors due to the small aerosol/molecular extinction ratio at these wavelengths. In particular, the spectral maxima between 300 and 400 nm should not be considered as physical features but result from the lack of instrument sensitivity to aerosols.'

2.13

Referee #2:

In any case, I see no clear justification that the new algorithm is doing better.

Author response:

We hope that our additonal correlation coefficients for the SAGE II/AerGOM comparisons convince the referee that the data have been improved (it's hard to neglect these

numbers). If not, we can only point the referee to the results of the accompanying paper.

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Discussion paper