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Atmospheric Measurement Techniques Discussions

Interactive comment on "Contiguous polarisation spectra of the Earth from 300–850 nm measured by GOME-2 onboard MetOp-A" *by* L. G. Tilstra et al.

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We would like to thank Dr. Liebing for performing a thorough review and for the many helpful suggestions to improve the paper. We respond to each of the review comments below. For the sake of clarity, the review comments are given in italics whereas our response is printed in normal font.

General comments:

However, the validation of the polarization sensitivity it is by no means "extensive", as

the authors suggest in the conclusion. An extensive (and comprehensive) validation would be if a rigorous and consistent statistical analysis had been performed on a large fraction of the data (if not all), rather than just a few selected examples. Given the uniqueness of these new data, I do think that the approach taken by the authors is for the moment sufficient to demonstrate the data quality. I would suggest, though, that the authors include a paragraph on why they have chosen this approach, and if possible, a statement on how the quality of the examples relates to that of a comparable, larger sample. This would prevent assertions that the selected examples have been chosen for their overly good performance compared to others.

The word "extensive" was badly chosen. What we wanted to emphasise in the paper was our goal to perform a validation using more than just one approach/technique. So, next to the comparison with simulations for (only) a few cases we made use of the independent special geometry method as well as of the backscatter geometry method. It is true that the last two methods only focus on situations where $Q/I \approx 0$. In principle, this only validates a subset of the GOME-2 PMD-RAW measurements. However, in a previous study it was shown (for the normal "banded" GOME-2 Stokes fractions) that the quality of these special geometry Stokes fractions is comparable to the quality of the Stokes fractions of all geometries.

In any case, we have deleted the word "extensive" from the paper and we now mention in the second paragraph of section 5 that the special geometries validation results are expected to be representative for all geometries, based on the results of this previous study. We have added a reference to the report of this study.

I am also missing a more profound discussion on the origin of spectral structures in the UV. If they are related to O3 absorption spectra, then why do they not show up in the simulated data? Also, in this case this may be very relevant information for DOAS-like atmospheric constituent retrievals in this wavelength region and therefore deserves to be discussed in more detail. Related to this is the impact of errors due to the low

spectral resolution of the normal PMD measurements on the radiometric calibration of GOME-2 spectra. A discussion on this may exceed the scope of this article, but even then I would suggest that an outlook on further investigation be included in the summary.

In the new version of the paper we now discuss the spectral structures that are seen in the UV wavelength range. They are indeed not caused by ozone absorption but by imperfections in the wavelength calibration of PMD-p versus PMD-s ("spectral misalignment") and by imperfections in the radiometric calibration of PMD-p and PMD-s.

We now also mention in section 7 and in the Conclusions (section 8) that further investigation should be made to explain the origin of these spectral features.

Note that the banded PMD Q/I (not discussed in this paper) are much less sensitive to errors in the wavelength calibration. On the other hand, as noted by the reviewer, the banded Q/I lack the spectral resolution of the PMD RAW mode and this of course also has an impact on the applied polarisation correction of the main science channel spectra.

Specific comments:

Section 3.1, page 11315, lines 12-19: I'd suggest to move this paragraph to the top of this section, in order to clarify that the article is about GOME-2 on Metop-A (is it?) and avoid confusion by mentioning all these different satellites.

We agree and have made the change as suggested.

Section 3.2, page 11316, line 4: The mentioning of OMI (and only OMI) feels a little unmotivated, I suggest to write "Other instruments such as OMI ..."

We agree and have made the change as suggested.

page 11316, line 14: Have any studies been performed yet to show that the instrument is indeed largely insensitive to U? One way to obtain a limit on the sensitivity of the PMDs at least is to separate the special cases of the left panel of figure 6 into cases with positive and negative U and look for systematic differences. However, this does not not necessarily need to be done for this paper, it's just an idea.

The sensitivities of the two PMD detectors to I, Q/I, and U/I have been determined during the on-ground radiometric characterisation campaign of the GOME-2 instrument. The resulting radiometric key data are available in the GOME-2 level-1a product and were used by us for the calculation of the Stokes fractions (and radiances and solar irradiances). The dependence on U/I is therefore fully taken into account. Changing the sign of U/I and checking the impact of this flip was one of the standard tests we used to make sure that the correct sign was indeed used. From this "flipping" it could be concluded that for a significant fraction of the geometries within an orbit the sensitivity to U/I cannot be neglected.

page 11317, lines 18-24: The referenced document (Munro and Lang 2010) is not, or not readily, available to the public. Therefore it would not hurt to include a comprehensive summary on the steps undertaken to obtain the calibrated Stokes fractions (and radiances?).

We agree and have included in section 3.2 of the paper a list of the steps that need to be taken to calculate the Stokes fractions and radiances.

The referenced document by Munro and Lang is an official EUMETSAT document and can be downloaded from the EUMETSAT website:

http://www.eumetsat.int/website/wcm/idc/idcplg?IdcService=GET_FILE&dDocName=pdf_ten_990011-eps-gome-pgs&RevisionSelectionMethod=LatestReleased&Rendition=Web

The reference in the bibliography of the paper now mentions the availability of this

document via www.eumetsat.int.

page 11317,lines 26/27: It is not clear from this sentence that the reflectance is also derived from the PMD signals rather than the spectrometer.

We agree. The sentence was rewritten to avoid confusion with the reflectance obtained from the main science channel measurements.

Section 4: Would it be possible to include an additional case over sea surface? I suppose it would be quite different at the red/NIR wavelengths and give some more information on the spectral features there?

No, it is not possible to provide reliable simulations over sea surfaces, at least not for the longer (NIR) wavelength. We deliberately restricted ourselves to scenes over land because the surface albedo over land is higher than that over sea (for the longer wavelengths). For land surfaces the contribution of the surface to the TOA radiance dominates the contribution by atmospheric scattering at the longer wavelengths.

For sea surfaces the surface contribution in the NIR is comparable to the atmospheric contribution. The surface contribution tends to depolarise, whereas the pure atmospheric contribution is highly polarised (single scattering). Small changes (i.e., inaccuracies) in the surface albedo lead to large changes (i.e., uncertainties) in Q/I.

In other words, the inaccuracies in the surface albedo over sea lead to large inaccuracies in the Stokes fraction for the longer wavelengths. This is why we cannot provide reliable/accurate simulations of Q/I over sea surfaces. This is also mentioned in the AMTD paper in section 5.1 (lines 17–21 on page 11322).

Section 4.1, figure 2: I suggest to plot this case 1 consistently with the other 2 cases, i.e., with the addition of the convoluted simulated spectra for the other 2 options for the LER.

[The figure that is meant here is Figure 3 on page 11335.]

We have followed this advice and have added the curves based on the GOME-2 and MERIS surface albedo databases to the figure. The figure is now consistent with the figures for the other two cases, with the exception that the “green” non-convoluted high resolution spectrum was not removed.

We did not remove this non-convoluted high resolution spectrum spectrum based on the GOME-1 LER because it helps explain the approach that was taken. Also, it illustrates the impact of the various absorption bands that need to be taken into account, as well as the high variation of the Stokes fraction Q/I inside these absorption bands.

The caption of Figure 3 has been updated and sections 4.1 and 4.2 have been partly rewritten.

We have attached the improved figure to this AC.

Section 5: For the left panel of figure 6 I would expect a similar effect in the UV-VIS as for the right one. At least my simulations indicate this. Can you explain why you do not see this large offset from 0 there, as opposed to the backscattering case. Could you perhaps include some (or a typical) simulation for the left as well?

[The figure that is meant here is Figure 7 on page 11338.]

Yes, a similar effect exists, but not as pronounced as for the backscatter geometries. We added a simulated Q/I spectrum for a typical special geometry situation in the left window of Figure 7. The caption of the figure and the description in section 5 have been changed accordingly.

We have attached the improved figure to this AC.

page 11323, line 27: The term "symmetry breaking" is very clearly defined as the destruction of an existing symmetry by a phase transition, so it should not be used in the

explanation of this feature. It is simply caused by an anisotropic distribution of multiply scattered light, and the effect is decreasing as the multiple scattering decreases (which is also what the simulation shows).

We have reformulated this paragraph of the paper. The term “symmetry breaking” is not used any more in the paper.

page 11323, line 33: I would rather give 0.01 as an accuracy margin.

The mentioned line 33 does not exist. On the mentioned page 11323 an accuracy margin of 0.01 is already given (in line 2).

Perhaps line 4 on the next page (11324) is meant. Here the paper mentions an accuracy of 0.005. We have changed this into 0.01.

Section 6, Figure 8: It seems to me that this spectral feature around 800 nm gets to be smaller with increasing polarization. Could it be that the feature is only in one of the PMDs?

Yes, this is certainly possible. Figure 8 seems to suggest that for low degree of polarisation (clouded scene) the feature is more dominant than for a higher degree of polarisation (cloud-free scene). Apparently the spectral feature is caused primarily by imperfections in the key data of one of the two PMD detectors.

Section 7: Here it would certainly not hurt to discuss the impact of the differences of fit vs. parametrization as well as O3 features on GOME-2 radiances.

This is now discussed in the paper.

Section 8: Would it be possible already to estimate the radiometric accuracy of GOME-2 due to polarization, given this new study?

Perhaps, but the limitations in accuracy caused by the performance of the polarisation correction have already been studied in the past on the basis of the normal PMD band Stokes fractions.

page 11328, line 7: As mentioned, the validation here is limited to a few examples with RTM simulations and special cases with hardly any polarization, so it certainly provides valuable information but is by no means extensive.

We agree and have performed the necessary changes.

Interactive comment on Atmos. Meas. Tech. Discuss., 6, 11309, 2013.

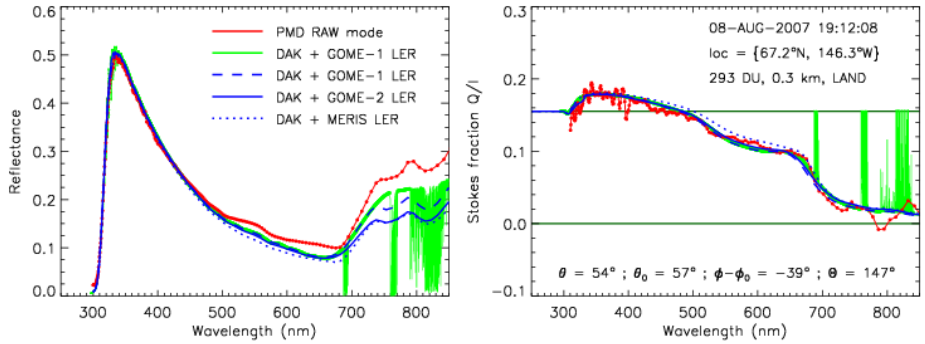

Fig. 1. Revised Figure 3.

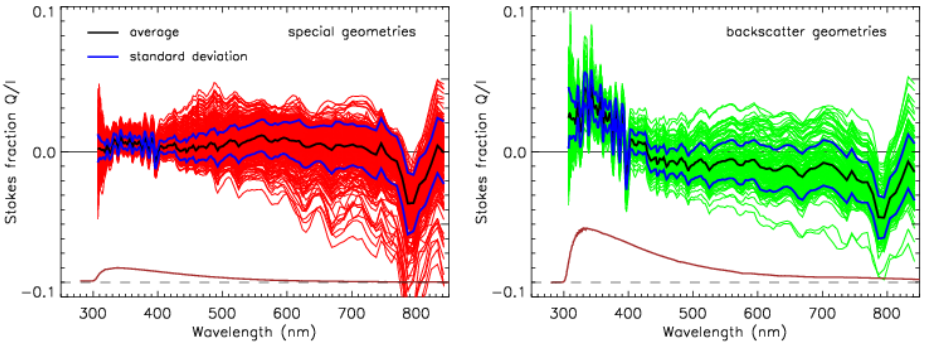

Fig. 2. Revised Figure 7.