amt-2015-118. Munro et al. "GOME-2 instrument on the Metop series of satellites: instrument design, calibration, and level 1 data processing – an overview".

Response to Anonymous Referee #1 Comments RC C3199

Thank you for the time taken to review the document and the helpful comments. Please note that the pages referenced to not equate to those in the discussion paper. They appear to correspond to a first version which was subsequently modified (including some reorganisation) in response to the associate reviewer comments, prior to publication for discussion. This complicates the response to the review somewhat but we will do our best to match the comments to the discussion paper.

Reviewer Comment (1)

The manuscript provides a very good compilation of the state of the knowledge on the GOME-2 instruments' design and performance. The results are well-structured and well-referenced and use good statistical analysis methods. There are good references to detailed reports for interested readers. The paper is generally well-written. Some questions and suggestions to handle them, some ideas to improve the flow and clarify some of the content in the tables and figures, and some minor editing notes are discussed below.

The Journal choice to place all of the tables and figures at the end of the document instead of near their descriptions in the text is inconvenient.

Response (1)

Thank you for your positive comments and the time taken to review the manuscript.

Reviewer Comment (2)

Page 10, Line 10. Are the SLS measurements able to give the relative changes (wavelength dependent ones) in the solar path throughput even without overall signal stability? That is, even though the lamp output is not stable, are the output variations sufficiently well correlated across the different lines?

Response (2)

Unfortunately the SLS lamp, when operated with long integration times, is also unstable in the spectral domain i.e. the variability is not well correlated spectrally. As a result the SLS lamp over diffuser measurements have not been as useful as originally planned and can only provide a rough estimate of the upper boundary for diffuser degradation.

Reviewer Comment (3)

Table 2. It is not clear in Table 2 that the last row of values is for both PMDs.

Response (3)

Agreed – this has become more unclear during type-setting due to the line positioning. This will be improved.

Reviewer Comment (4)

Table 3. This table has designations for two footnotes "*" and "1" but only one footnote is present.

Response (4)
Agreed – this is a typesetting error. There should only be one footnote. This is footnote "1". This will be corrected.

Reviewer Comment (5)
Page 6 Line 19. There should be some clarification here. Maybe "For Earthshine measurements, subsets of the 256 detector pixels of both PMD devices (for details we refer to EUMETSAT, 2014b) are selected and co-added spectrally on board to create 15 broader PMD spectral bands to send to the ground. The numbers and spectral locations of the pixels are provided in Tables 4, 5 & 6."

Response (5)
Agreed – the text has been updated.

Reviewer Comment (6)
Page 6, Line 14. Minor edits. "A sub-set of the calibration key data is a required input to the GOME-2 level 0 to 1 processor, e.g., the radiance, irradiance and polarization responses of the instrument." Note: "e.g." is an abbreviation for "for example" in Latin. As such it should be italicized. If it is used to start a clause, then it should be offset with at least one comma from the main part of a sentence.

Response (6)
Comma added. The italicization of e.g. is limited by the English guidelines of the journal where it is stated "Common Latin phrases are not italicized (for example, et al., cf., e.g., a priori, in situ, bremsstrahlung, and eigenvalue)."

Reviewer Comment (7)
Introduce Etalon. The term "Etalon" is first used on Page 5, line 27 and then three more times through Page 13 without any discussion or promise of further illumination on the phenomenon. This is okay for an instrument effect like stray light but the novice GOME-2 reader needs some guidance or at least the assurance the all will be explained in Section 3.5.

Response (7)
Agreed – a reference to Section 3.5 is added after the first occurrence of "etalon"

Reviewer Comment (8)
Page 14, Line 5. Add the following notice: "Cloud parameters are also determined and added to the output. The methods to determine these parameters are described in Section 3.9."

Response (8)
Agreed – text updated.

Reviewer Comment (9)
Page 15, Line 3 and following. While it is certainly important for users to be aware of the spatial aliasing and how to calculate adjustments, this material breaks up the overall discussion and seems to have too much detail relative to the surrounding content. I recommend that the detailed description of the calculations and the equations be moved to an appendix.

Response (9)
The point is well taken, however it is very important that this information is well understood by users in order that they use the data correctly in higher level product derivation. There is a risk that if it is moved to an Appendix it will be overlooked. For this reason I would prefer to leave this section in the main body of the paper.

Reviewer Comment (10)

Page 19, Section 3.4. The instrument bandpasses will be differentially impacted by wavelength-dependent throughput gradients in any dichroic or other reflective/transmissive discriminating element. Consider a grating/detector system that produces a balanced symmetric Gaussian bandpass convolved with a decreasing throughput dropping by 10% across the nominal bandpass FWHM, say from 22% to 20% transmission. This will produce a skewed bandpass resulting in shifts in the bandpass weighted average wavelengths. Further, the gradients have opposite signs for two channels around their overlap point. Are these bandpass shape/skewing variations observed for GOME-2? The relative variations become largest for wavelengths with the largest relative changes in the throughput. This usually occurs where the dichroic is delivering 50% or less of the signal.

Response (10)

The pre-flight characterisation of the GOME-2 slit function/instrument spectral response function (ISRF) focused on the valid range of the detectors (used for deviation of level 2 – geophysical - data products) which typically excludes the overlap region between two channels where the transmission is below 50% so the information requested is not available. It is however known that the degree of asymmetry of the ISRF varies in the spectral domain.

Reviewer Comment (11)

Figures 8 and 9. The information in the legends for Figures 8 & 9 could be repeated in the figure titles since the fonts are very small.

Response (11)

Agreed – Figure caption will be updated.

Reviewer Comment (12)

Page 22, Section 3.6. To me, a full calibration would mean removing the effects of instrument throughput degradation. Since the solar and earth spectra are left with a shared component, we are given an in-between quantity – the radiance at the spacecraft adjusted for the ground-based characterization of throughput but not all of the ensuing known changes in orbit. I am not sure what term to use in place of "full". This would be proper usage for the reprocessed Level 1 data.

Response (12)

The calibration activities described in this section would lead to a fully calibrated solar spectrum in the absence of instrument degradation and certainly provide a fully calibrated instrument at beginning of life. I think it is important to separate the fundamental calibration activities and the effect of in-orbit instrument changes. None-the less the point is well taken. I would suggest removing the word "fully" as a compromise solution.

Reviewer Comment (13)

Figures 9 and 10. Perhaps a scaled "minus T" curve from the average for Figure 10 could be superimposed on one of the Figure 9 plots to good effect for displaying the correlation between the temperature evolution and the wavelength scale variations.

Response (13)

Whilst we understand the intention we fear that this may lead to a very busy plot which in the end may be difficult to interpret.

Reviewer Comment (14)
Figure 11. The surface plots should be stretched in the Z direction as that is the main result. Consider removing the internal titles and the top figure's solar azimuth angle axis label. A simple (A) and (B) in the plots with references in the title can identify the two cases.

Response (14)
Agreed – will be done.

Reviewer Comment (15)
Figure 14 and Page 31, line 14. Were solar cycle activity features (27-day variations) removed from the data for the 250 nm to 290 nm region? Are there any theories on the causes of the vertical striping for 300 to 400 nm range most apparent in the PMD-S figure but also present in Band 2 near 350 nm?

Response (15)
In this figure which is largely illustrative, no. This could be done, however this figure is very dominated by the overall throughput loss at these wavelengths so the difference may not be visible. When analysing the solar spectra for the purposes of derivation of a degradation correction, such effects are taken into account. This will be the subject of a future more detailed paper. See comment below. The exact origin of the vertical striping is not clearly known. They seem to be a result of degradation since they evolve with time. Also one can see them when comparing an old instrument (Metop-A) to a new instrument (Metop-B) in the old instrument, and remove them when subtracting a empirical degradation vector from Metop-A. However spectral sampling effects cannot be completely excluded. These features are accounted for in the derivation of a degradation correction.

Reviewer Comment (16)
Page 32, Lines 1 to 3 and Lines 26 to 27. Where and when will this quantitative analysis appear? What locations are used to monitor the year-to-year changes? How do these adjustments compare to the information from Level 2 product developers?

Response (16)
It is now yet known exactly when and where this detailed information will be published but the target would be in 2016. The locations used are a selected area of the Sahara, the Pacific and ice sheets. The evaluation loop with level 2 product developers is currently on-going.

Reviewer Comment (17)
Section 4, Level 2 feedback. It would be nice to have more information (or at least citations) on the feedback on calibration provided by Level 2 product developers. Possible topics/references for Section 4 would include: Trends in trace gas retrieval uncertainty from decreased throughput; Trends and cross-track changes in UV Absorbing Aerosol Index values (or the adjustments made to remove them); trends in adjustments applied by GOME-2 ozone profile retrieval product creators; and sizes of intra-orbital wavelength scale shifts estimated from DOAS retrievals, and, perhaps, their relationship to intra-orbit thermal changes and Figure 9 & 10 long-term results.

Response (17)
Whilst this would be an interesting addition I see two problems in including information in this particular paper – first the scope is very large and it would increase the length of the paper (which is already quite long) considerably, and second, this information is provided informally to the authors of the current paper but it is not readily available in citable peer-reviewed publications. The corrections needed by level 2 algorithms are also intimately related to the design of the algorithm itself so it is

difficult to discuss without a description of the underlying algorithm which is out of the scope of this paper.