Response to comments on egusphere-2025-4639 Submitted Sep 2025

Anonymous Referee #2

Referee comment on "**Comparative analysis of GOME and SCIAMACHY reflectance over Pseudo-Invariant Calibration Sites: implications for spectrometers cross-calibration**" by Owda, A., and Lichtenberg, G.
https://egusphere.copernicus.org/preprints/2025/egusphere-2025-4639/

We are grateful to the anonymous referee for taking the time to review our manuscript and provide us with constructive comments. We have carefully considered all comments and suggestions and thoroughly revised the manuscript.

In this response letter, we intend to address all comments, concerns, and suggestions point by point. The response will be highlighted in blue and the corresponding modification in the revised manuscript in orange. The original comment from the anonymous referee will be in black.

General comments:

"Summary of the paper

This paper addresses the (top-of-atmosphere) reflectance stability over 20 pseudo-invariant calibration sites, based on the GOME and SCIAMACHY instruments. The purpose is to extend the use of PICS from imagers to spectrometers by developing a statistical framework for comparison and application of this to the selected PICS.

The method used to obtain these objectives is statistical analysis of the extensive nearly decade-long overlapping timeseries of GOME and SCIAMACHY measurements, after correction for solar zenith angle and viewing zenith angle variations for the selected PICS, and filtering on proximity to the pics and cloud fraction. The corrected reflectance for selected wavelength ranges in the UV, visible and NIR is then statistically analysed resulting in a range of metrics.

The key results and conclusions are the definition of a robust framework for sensor performance evaluation, radiometer drift monitoring, and identification of the constrained and reference sensors for cross-calibration. The stability of the

20 PICS was assessed using a Stability Score, resulting in a ranking of the PICS and identification of 3 PICS that were observed to be most stable."

Response: Thank you so much for the positive impression of our research paper. This is exactly the purpose of our paper. We will also address the important point about using TOA reflectance in the manuscript.

We have dealt with your comments one by one as follows:

Major comments:

***ToA versus surface reflectance*. From the description of the data processing, it is clear that the top-of-atmosphere (ToA) reflectance is used for the analysis. For PICS use with imagers on the other hand, it is quite common to use surface reflectance or -BRDF, with a corresponding atmospheric correction applied to remove the impact of the atmosphere.**

The main concern with the approach taken in the paper is that the distinction between atmospheric variability and surface reflectance variability is difficult to make, if at all possible. From the perspective of a user of PICS for imaging applications the coefficient of variation may seem excessively high. From the perspective of a user focused on the Earth atmosphere the numbers are not at all surprising.

I suggest addressing the difference between ToA and surface reflectance quite early in the paper to make sure that reader is aware of this difference. Likewise, in the discussion on the stability of PICS this difference will need to be properly addressed; direct comparison of the ToA reflectance stability with surface reflectance stability can not be done without carefully addressing the atmosphere and/or atmospheric corrections.

[Fixed]

Thank you for this important point. We agree that it is necessary to clearly state that TOA reflectance is used, even though this was already mentioned in the data description.

For our use and for the second paper, which is the companion paper of the study, "https://egusphere.copernicus.org/preprints/2025/egusphere-2025-4942/", it was necessary to use TOA for the following reasons:

1) **We perform the cross-calibration based on TOA reflectance. Therefore,**

2) **We need to know the radiometric differences in the sensor by performing inter-comparison based on TOA reflectance. Which sensor should be used as a reference?**
3) **We selected the most stable sites to use for the cross-calibration based on TOA reflectance.**

We acknowledge that, for imagery users and the broader remote sensing community, coefficients of variation (CVs) and other statistical parameters are strongly influenced by atmospheric composition. Some parts of the spectrum, particularly at longer wavelengths, are less influenced by the atmosphere and can describe the surface homogeneity better than short wavelengths.

Separating surface and atmospheric contributions is essential. We will consider that in a separate forthcoming paper.

Based on this comment, we have revised the manuscript accordingly and clarified the use of TOA reflectance.

In the abstract

Line 10: "Decades of top-of-atmosphere (TOA) reflectance data in the…"

In the introduction, as you suggested, have an early introduction about the differences between TOA and surface reflectance, and what is expected to have:

Line 10: "Decades of top-of-atmosphere (TOA) reflectance data in the ultraviolet, visible, and near-infrared (UV/VIS/NIR) ranges, collected by the Global Ozone Monitoring Experiment (GOME) and Scanning Imaging Absorption Spectrometers for Atmospheric Chartography (SCIAMACHY) spectrometers over 20 PICS sites, were analyzed."

***SZA and VZA corrections and the slope parameter*. Solar Zenith Angle and Viewing Zenith Angle corrections are considered during data processing. Figures 10a and 10b show the mean reflectance as function of these parameters for all selected PICS observations. Based on these plots the conclusion is drawn that only SZA dependence is corrected.**

Do I interpret this correctly?

[Fixed]

In the revised manuscript, we have corrected for both SZA and VZA. We used a simple linear regression model to correct this dependency (or detrending the time series).

From basic principles, I would expect a Lambert-Beer-like behaviour for each wavelength as function of the airmass of the combined path from sun to surface to satellite, assuming that the majority of the light reaches the surface and the atmosphere is homogenous. In the case of strong absorption (e.g. ozone, oxygen, high aerosol loads, Rayleigh scattering at short wavelengths) this assumption may not be valid. Likewise, variability of atmospheric constituents will increase the scatter. A Langley plot (or the equivalent thereof for the observation of the Earth surface from orbit) should indicate whether correction is need or can be neglected for the wavelength in question.

Taking this one step further, the metrics would probably be able to better distinguish between atmospheric variability, PICS surface variability, and instrument variability if SZA and VZA airmass dependence were taken into account with an additional wavelength-dependent atmospheric absorption coefficient, as well as the instrument-dependent slope parameter. Note that from the Level 2 products the atmospheric composition is known for each observation, in case that ozone variability is to be taken into account.

Thank you for this insightful comment. We agree that, from first principles, TOA reflectance is expected to exhibit a Lambert–Beer–type dependence on atmospheric airmass, with deviations occurring in spectral regions affected by strong absorption or enhanced scattering. In this study, we explicitly evaluated the dependence of TOA reflectance on solar zenith angle and viewing zenith angle.

While a full Langley-type analysis, including wavelength-dependent atmospheric absorption coefficients and viewing geometry, could further separate atmospheric, surface, and instrumental contributions, such an approach is beyond the scope of this work, which focuses on relative temporal variability and sensor consistency. We note, however, that the increased scatter observed in specific spectral regions is consistent with enhanced atmospheric influence and is taken into account in the interpretation of the results.

***Instrument degradation and scan angle dependence*. Both GOME and SCIAMACHY suffer from instrument degradation, with a non-negligible component of that being scan angle dependent degradation. Both instruments have monitoring capabilities and correction for degradation during level 0 to level 1 processing, but this correction is not perfect. Especially for SCIAMACHY, the quality of the degradation correction depends on the version of the processor and corresponding calibration key data.**

Please report briefly in section 2.3 which version of the data was used.

[Fixed]
SCIAMACHY v.10 and GOME v5.1. The data versions were added to the manuscript.

Line 87: "Fully calibrated Level-1 products from GOME (Level-1b version 5.2) (ESA, 2025a) and SCIAMACHY (Level-1c version 10) (ESA, 2025b) were used to derive TOA reflectance time series."

In view of the scan angle-dependent degradation, have you looked at the PICS reflectance time series as a function of the scan angle? Figure 5 suggests three "lines" for the 330 nm and much of the 450 nm time series. Do these happen to correspond to the East, Nadir, and West pixels? If so, does this affect your conclusions?

Thank you for the insightful comments. We have examined the reflectance as a function of scan viewing angle and observed a stronger dependence for GOME than for SCIAMACHY. This dependency is most pronounced in the UV bands, which is consistent with the more severe radiometric degradation observed in the UV compared to other spectral regions. Several studies reported similar findings and agree with ours as well.

This effect has been explicitly considered in the cross-calibration of GOME and SCIAMACHY and is discussed in detail in our second submitted paper. The

preprint version of that work is available at the following link: https://egusphere.copernicus.org/preprints/2025/egusphere-2025-4942/egusphere-2025-4942.pdf

In particular, Figures 5 and 7 illustrate that the three parallel lines arise from differences in scan viewing angles. We do not observe a comparable dependency in the VIS and NIR bands, supporting the conclusion that the majority of the degradation effects are confined to the UV spectral region. This means that the 3 parallel lines in the 330 nm time series plot (Fig.5) come from the different scanning viewing angles.

We updated the manuscript by describing this pattern on UV based on our analysis from the companion paper.

Line 211: "Additionally, the reflectance time series exhibited three clearly separated clusters, likely resulting from the dependence of reflectance on VZA.

***Seasonal dependence*. Section 5.4 mentions clear seasonal dependence with the largest correction values during the summer months. This happens to be the months with highest aerosol load at many desert sites. Have you looked at the correlation between the absorbing aerosol index product or aerosol optical depth and the observed seasonal dependence?**

No, we have not looked at the aerosol index data. However, that is expected, and I have already cited and added some references about the impact of aerosols. E.g.,

Line 437: "The UV bands are highly sensitive to atmospheric processes and aerosols (Chatzopoulou et al., 2025; Li et al., 2012)."

Line 359: "Wang et al. (2022) reported that reflectance tends to increase with both the atmospheric aerosol optical depth and SZA."

Looking at figure A1, the heatmap of the difference in reflectance, clear seasonal variability of ozone can be seen in the typical Huggins band spectral features in the UV. Likewise, the oxygen A-band shows up clearly in the spectral behaviour of the time series. This clearly points at atmospheric effects being a large contributor to the observed variability.

We have to mention the following:

First: We have unified the reflectance for a reference geometry of a SZA (45 degrees) and VZA (0 degrees). In the summer months, the SZA is lower than in winter months; therefore, there will be more correction for the summer months (when we correct to SZA 45). This is the first reason why we have the maximum correction in the summer months. A major part of this correction refers to the reference geometry of observations.

Second: The observed seasonal dependence, with larger correction values during summer months, coincides with periods of enhanced aerosol loading over many desert calibration sites. Increased dust activity during summer is known to affect TOA reflectance, particularly in the UV spectral region.

While a detailed quantitative attribution of the seasonal signal to aerosol variability is beyond the scope of this study, the timing and spectral characteristics of the observed dependence are consistent with enhanced atmospheric influence. The potential role of aerosols has been acknowledged in the revised manuscript and is considered in the interpretation of the results.

[Fixed]

Line 365-368: "Using a reference geometry with SZA = 45◦, the typically lower SZA values during summer lead to increased reflectance after correction to the reference geometry compared with winter. In addition, the magnitude of the correction is wavelength dependent, with the largest corrections observed in the UV bands. This behavior was likely associated with enhanced atmospheric transport of aerosols and dust during summer. Previous studies have reported a pronounced seasonal dust cycle, characterized by higher dust concentrations in the summer months (Rezaei Nokandeh et al., 2025). Furthermore, both the central Sahara Desert and the Arabian Peninsula experience their highest aerosol optical depth during summer (Logothetis et al., 2021)."

You state that the required adjustments were minor, on the order of 0.05. With a typical mean reflectance of 0.10 to 0.30 in the UV to visible, the correction of 0.05 however corresponds to 15% to 50% of the typical reflectance. This is not directly minor. Please elaborate. This is also reflected in the coefficient of variation, which varies between 0.10 and 0.25 for wavelengths in the UV-vis.

[Fixed]

Yes, we agreed that the amount of correction is not minor, as the range of reflectance in the UV is between 0.10 and 0.30. We decided to remove the "minor" from the discussion

Minor comments and typos:

Line 48: **"spatial footprint […] of PICS". Do you mean spatial extent? Please rephrase or clarify.**

[Fixed]

Yes, spatial extent. It has been paraphrased.

Equation 13, line 172: **What is the index j? And what happened to the wavelength lambda? Please clarify/correct.**

[Fixed]

$$\overline{SS_S^{S_k}} = \frac{1}{N}\sum_{j=1}^{N} SS_{S,\lambda}^{S_k}$$

The equation describes how the overall stability score of the PICS site is computed. By averaging all scores of all wavelengths of the whole spectrum (UV/VIS/NIR) used in the paper. Additional details about the equation are added.

where j is the number of spectral channels, SSSk
S,λ is the stability score of a spectral channel of a wavelength λ

Line 185-186: **Since the degradation of both GOME and SCIAMACHY is corrected to some degree, "less degradation" could be better formulated as "better degradation correction".**

[Fixed]

Line 188: **Should 650 nm be 450 nm? Please correct.**

Yes, it is corrected

[Fixed]

Line 156-157, Table 3, and line 210-211: **is the slope parameter m the same in these cases? If so, do you have a fit parameter for time dependence? If not, please clarify and update.**

[Fixed]

Yes, on lines 156-157 and *m* in Table 3, they are the same—the slope of the reflectance time series with respect to time.

It was fitted as a linear regression model. The parameters are available (but not shown in the paper).

Line 214: **Could aerosols have an influence as well as clouds?**

[Fixed]

Definitely, therefore, we have updated the manuscript with the potential influence of aerosols, particularly the desert dust aerosols in the summer in the Saharan Desert.

Line 224: **The "slight changes in the spectral signature of the sites" of the slope parameter is a remarkable feature that I do not directly expect or understand. The spectral behaviour of the slope parameter in figure 8e remind me of the detector etalon of SCIAMACHY (sinusoidal pattern) and around 350 nm of a detector feature that was already present during the on-ground calibration phase and which increased slowly over time in size on the detector. Apart from polarisation dependence and perhaps spatial non-uniformity on a scale of the projected length of the slit I would not expect site-dependent variation.**

The oscillations in the slope (8e) of the UV channel is possibly caused by residual etalon changes. The dip around 350nm is likely caused by stronger degradation of the band 2 detector. This is a known feature."

Line 245: The dip around 350 nm was likely due to stronger degradation of the Band 2 detector, which was a known characteristic of the instrument.

Line 233: **In addition to stable sensor data, the effects of illumination and viewing geometry are also of high importance. Think also about the Local Time of**

Ascending Node (LTAN) crossing (overpass time) when comparing results between sensors. In this case, ERS-2 and Envisat have quite similar and stable LTAN.

This information is important, especially for the cross-calibration we performed for GOME-1 based on SCIAMACHY as a reference. The local time difference at the equator is about 30 minutes. They have a close viewing and illumination angle and could also have similar atmospheric conditions within these time differences.

***Figure 7e and 8e*: add more decimals to the vertical axis, they show only zeroes now.**

[Fixed]

***Line 254*: What is the definition of a spectral channel? GOME and SCIAMACHY naming suggest one of 4 (GOME) or 8 (SCIAMACHY) spectrometer channels, each consisting of 1024 spectral pixels. Other more modern definitions denote individual detector pixels with the term "spectral channel". Please define or clarify.**

I mean, with the spectral channel, the individual pixel. If a band has 1024 pixels, then each pixel is a spectral channel with a wavelength. This is the concept we use in our paper.

Line 122-123: "In this paper, the term 'spectral channel' referred to an individual spectral pixel of the detector, whereas 'spectral band' denoted the detector channel."

***Line 258*: Typo: "sereis" -> "series".**

[Fixed]

***Line 259*: The aerosols in the summer months (May-September) may exhibit in addition to radiometric effects also polarisation effects. The polarisation effects of GOME and SCIAMACHY have distinct spectral structures and may slowly change over time due to instrument degradation. I expect this to be a small effect, however.**

Yes, we agree on that. In our analysis, we neglect that effect

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***Figure 10b*: Is this the viewing zenith angle dependence, or does it show something else, e.g., the viewing azimuth angle? The range from approximately 100 to 300 degrees is not consistent with zenith angles. Please update.**

[Fixed]

100-300 degrees is the range for viewing azimuth angle (VAA), not for VZA. The figure is corrected by showing the VZA. Furthermore, the correction for TOA reflectance was applied for SZA and VZA.

See Fig. 10(b).

***Figure 12*: alignment of the Mauretania1 and Mauretania2 names is shifted, please update.**

[Fixed]

See Fig. A3 in the Appendix