

Dear editor,

We re-submitted our revised manuscript. We found the reviewer comments very helpful and constructive and addressed them all as outlined below.

Best regards,

Thomas Wagner

Anonymous Referee #1

Author replies are shown in blue

New text is shown in red

The paper "Characterisation of spectroscopic properties of DOAS instruments using high-resolution solar spectra" by Thomas Wagner et al., presents a comprehensive investigation of spectroscopic properties of DOAS instruments using a Kurucz-fit approach applied to a long-term zenith-sky dataset from Kiruna, Sweden, spanning 30 years. The study investigates the impact of a detector change from a photodiode array to a CCD, demonstrates that the Ring effect leads to a systematic broadening of the retrieved ISRF, with a strong dependence on cloud conditions and seasonal changes in surface albedo. The authors provide also recommendations for the determination of the ISRF, intensity offsets and the wavelength dependence of the light throughput of the instrument. Continuous monitoring of the instrument properties is very important, particularly for long-term trend analyses, where instrumental effects can otherwise introduce biases.

The paper is well written, clearly structured and easy to follow, and its scientific content fits the scope of AMT. Below are my review, comments and remarks.

Many thanks for the positive assessment and helpful comments, which we all addressed as outlined below.

General comments:

(1) The authors state that the spectrometer of the instrument is maintained at $30 \pm 0.1^\circ\text{C}$ (P.33, L.700). Nevertheless, could small temperature instabilities still contribute, at least partly, to the observed variability of retrieved instrumental parameters such as the FWHM? While the manuscript suggests that the dominant seasonal variability is driven by the Ring effect, an investigation of the diurnal variability of the retrieved instrument parameters during clear-sky days might help to assess the magnitude of (if any) remaining temperature-related contributions.

We can not exclude that there are small remaining temperature instabilities, but we found no obvious temperature deviations on a diurnal or seasonal basis. As suggested, we checked the diurnal variation of the FWHM on clear days, but found only a systematic dependence on the SZA, which is caused by the SZA-dependence of the Ring effect (see e.g. Fig. 5). Besides that, no further dependency of the FWHM was observed.

This information was added to section A1.

(2) Can the authors comment to what extent their main conclusions and recommendations apply to lower-latitude sites with weaker but still variable surface albedo and to other viewing geometries (e.g. MAX-DOAS)?

We added the following text as new point (e) to the conclusions:

‘e) Measurements at lower latitudes (without the presence of snow in winter) will be less affected by the seasonal changes of the Ring effect, but for measurements during twilight, the RSP at 90° SZA is still rather high (for a surface albedo of 0.05 it is about 0.075 for 350 nm and about 0.05 for 440 nm). Fortunately, for albedo values below about 0.1 the dependence of the RSP on surface albedo becomes almost negligible (changes are below about 0.002 at 320 nm, and below about 0.001 for wavelengths > 400 nm, see Fig. A6). For MAX-DOAS measurements, the situation is more complex than for zenith-sky observations, because the Ring effect depends not only on SZA, but also on the elevation and relative azimuth angles as well as on the aerosol load (Wagner et al., 2009b). However, since MAX-DOAS measurements are usually analysed for smaller SZA ranges than zenith-sky observations, in general the strength of the Ring effect is weaker than for zenith-scattered light during twilight (see Fig. A6). Moreover, for MAX-DOAS measurements the focus is usually on trace gases located close to the surface (and thus typically below the last molecular scattering event), and their absorptions are hardly affected by the broadening due to rotational Raman scattering. Thus, to obtain the ISRF for the convolution of the trace gas cross sections the Raman scattering contribution should be subtracted from the measured spectrum before a KF is performed. Since for longer wavelengths, the strength of the Ring effect decreases, also a clear-sky zenith spectrum at low SZA might be selected for the Kurucz fit, for which the Ring effect is rather small.’

We also added a new figure showing the dependence of the RSP on SZA, surface albedo and aerosols (Fig. A6) to the appendix:

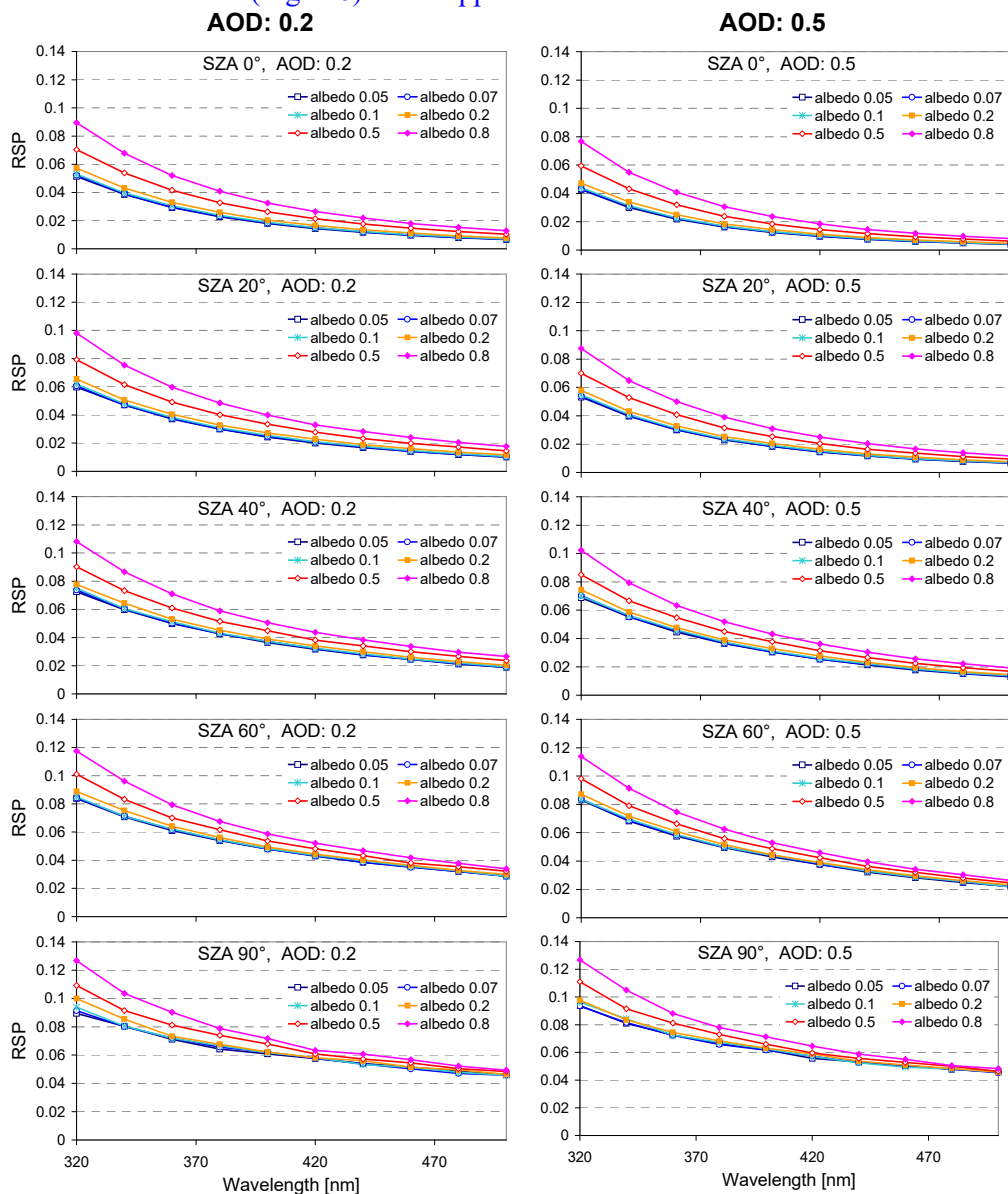


Fig. A6 Dependence of the RSP on wavelength for different surface albedos, SZA and aerosol loads derived from radiative transfer simulations with the radiative transfer model MCARTIM (Deutschmann et al., 2011). The aerosol profile is assumed as box-profile from the surface to 1 km. Single scattering albedo and asymmetry parameter are set to 0.95 and 0.68, respectively.

(3) In an ideal spectrometer the spectral line shape is determined by the grating, slit, and optics, while the detector merely samples the spectrum. The authors demonstrate that the older PDA detector is affected by the Fabry–Pérot etalon effect, which alters the sampled spectrum and influences the effective ISRF. Is this the only detector-related parameter that is different between the PDA and CCD sensor used in the study that may affect the ISRF?

The quantum efficiencies of both detectors are also different. However, the wavelength-dependencies of both detectors show no ‘narrow’-band spectral features such as the Fabry–Pérot-etalon effect. Besides that, we see no further systematic

differences. We added the potential effect of the wavelength-dependent quantum efficiency under point i) (old point h) in the conclusions.

(4) The authors conclude that, in order to obtain reasonable and spectrally consistent results of the ISRF FWHM, neither an intensity offset nor a Ring spectrum should be included in the KF. I think the authors should comment on whether this is expected to be an instrument-dependent conclusion and/or and if the same behavior is expected for different spectral regions (e.g. in the visible)

The following information was added to point c) in the conclusions:

‘These conclusions are derived for our zenith-sky DOAS measurements in the UV spectral range. Further studies should be carried out for different instruments and wavelength ranges. Possibly, for instruments with higher spectral resolution, the effects of rotational Raman scattering and the width of the ISRF could be better separated.’

Specific comments:

P.3, L.71: The Fabry–Pérot etalon effect is first introduced at this point. While a short description of the effect is given in Sect. 4.2 (L.411-412), I think such a description is more appropriate here.

We added the following information to the text: ‘The Fabry–Pérot etalon effect (Pérot and Fabry, 1899) is an interference effect (here between reflecting surfaces on top of the detector) and will be discussed in more detail in Sect. 4.2.’

P.4, Fig. 1: What do IRF 1 and IRF 2 represent? The small hut on the roof and the room inside the institute, respectively?

Yes, correct. We added this information to the figure caption.

P.5, L.117-120: Can the authors comment on how the KF performs at lower SZAs? Are similar results expected?

We added the following information at the end of the paragraph (directly before Sect. 3.1): ‘For smaller SZA, similar results of the KF are obtained, but the derived FWHM is usually (for clear days) smaller, because the strength of the Ring decreases with decreasing SZA (see also Sect. 3.3).’

P.5, L.137-138: Is an ozone absorption cross section included in the fit as stated in L.110? Please clarify

We made clear (in the second sentence of Sect. 3) that for all cases an ozone absorption cross section was included.

P.7, L.154-157: Can the authors comment on why doesn't the inclusion of a Ring spectrum and/or of an intensity offset improve the results, especially in the UV range? Is this an instrument-dependent observation?

With respect to the first question, we added the following information at the end of Sect. 3.2: ‘This finding is further explored and discussed in the following 2 sections.’ With respect to the second question, we added the following text to point c) in the conclusions (see also our reply to point 4 above): ‘These conclusions are derived for our zenith-sky DOAS measurements in the UV spectral range. Further studies should be carried out for different instruments and wavelength ranges. Possibly, for instruments with higher spectral resolution, the effects of rotational Raman scattering and the width of the ISRF could be better separated.’

P.8, Fig. 3b: Are there any missing data in panel b) between 2003 and 2007? Or is this due to a visualization reason?

Many thanks for this hint! The data were lost during combining the time series of the individual years. The missing data were now added.

P.13, Fig. 5: The simulated RSP (panel a) should either become differential RSP, relative to 80 deg. SZA, or for the measured RSPs (panels b and c) the ylabels should be dRSP

We changed the labels in panels b and c to 'dRSP'

P. 20, L.309-400: Are there any lamp measurements to confirm the increase of the FWHM?

Unfortunately, no atomic line lamp measurements during that period are available. During the next visit at the measurement site (hopefully in 2026), atomic line lamp measurements) will be performed.

Technical corrections:

P.3, L.61: "ist" -> "is"

corrected

P.4, L.104: "ERS" -> "ESR"

corrected

P.4, L.109: "software QDOAS" -> "QDOAS software"

corrected

P.6, L.149: "Finally, also the effect...". -> "Finally, the effect..."

corrected

P.7, L.160: "Mio spectra" -> "million spectra"

corrected

P.8, Fig. 3 caption: Color assignments are wrong. They should probably be magenta, cyan, orange and black. Please revise. Same applies for Fig. 14 and Fig. A4

The figure caption of Fig. 14 was corrected. For Fig. 3 and Fig. A4, the color assignments in the figure captions were removed.

P.9, L.206: "prpared" -> "prepared"

corrected

P.9, L.207: "teh" -> "the"

corrected

P.9, L.216-217: "According to Wikipedia (2025), snow cover generally lasts from late September to mid-May". This sentence may be omitted since afterwards, the snow depth is given in Fig. 4a

the sentence was deleted

P.10, Table 1 caption: "Also shown are the settings from Alliwel et al., (2002)" -> "The settings from Alliwel et al., (2002) are also shown/included"

corrected

P.11, Fig. 4: The x label names (month names) should be given in English. Same applies for Fig. 13. Also in panel d, the marker color of the legend for 341-348 nm is wrong
corrected

P.12, L.241: “Kuruzc” -> “Kurucz”
corrected

P.12, L.252: “...are shown (black dots)” -> “filled markers”?
corrected

P.12, L.253: “Fig, 5a” -> “Fig. 5a”
corrected

P.12, L.255 “(blue dots)” -> “(small dots/markers)”?
corrected

P.15, Fig. 7: A ylabel should be given
corrected

P.15, Fig. 7 caption and in all other places: “ISFR” -> “ISRF”
corrected

P.16, L.311-312: “the better choice” -> “the best choice” or “a better choice”
corrected

P.16, L.315: the word “however” is not necessary
corrected

P.16, Table 2 caption and in all other places: “super Gaussian” -> “super-Gaussian”
corrected

P. 20, L.408 “charateristics” -> “characteristics”
corrected

P. 21, L. 437: “Appendix 3” -> “Appendix A3”
corrected

P. 33, L.698 and 700: “spectromter” -> “spectrometer”
corrected

P. 33, Fig. A1 caption: Either include a) and b) texts in the two panels or replace with “left”-“right”. Also replace “the visible spectrometer” with “the spectrometer operating in the visible range” or something similar.
corrected

P. 41, L.797: “of scattered” is a duplicate
corrected

Anonymous Referee #2

Author replies are shown in blue

New text is shown in red

General Comments

This manuscript evaluates spectroscopic properties of zenith-sky-viewing DOAS instruments by extending the Kurucz fit method using high-resolution solar spectra. The method is applied to 30 years of UV-visible zenith-sky spectra recorded at Kiruna, Sweden, to characterize the dependence of instrument properties (instrument spectral response function, intensity bias, light throughput) on different observing conditions, and to assess the effect of changing from a photodiode array to a CCD detector. The manuscript clearly states the primary findings and gives specific recommendations for applying the Kurucz fit. A major conclusion is that the Ring effect causes a systematic broadening of the width of the instrument spectral response function derived from the Kurucz fit compared to that derived from mercury lamp measurements. The strength of the Ring effect is found to change when optically thick clouds are present and to have a seasonal dependence due to the surface albedo. The manuscript also presents a method for correcting wavelength dependence of the light throughput (such as that caused by the Fabry–Pérot etalon effect) using the results of a modified Kurucz fit.

The work is clearly presented and will be of interest to those involved in UV-visible spectroscopy and DOAS measurements of atmospheric trace gases. I have only a few minor comments to add to those of Reviewer 1, and I recommend publication after both sets of comments are addressed.

Many thanks for the positive assessment and helpful comments, which we all addressed as outlined below.

Specific Comments

Page 7, line 170: Text refers to Figure A3 on page 35 as showing variations of the strength of the Ring effect due to the varying cloud cover, but the figure shows FWHM vs. RSP. It is not clear from the figure or caption how this is related to varying cloud cover – explain.

We added a detailed discussion of the effect of clouds on the Ring effect in section 3.3. In that context, we removed the reference to Fig. A3 on page 7 and changed there the text to:

‘This finding is caused by variations of the strength of the Ring effect due to the varying cloud cover (see e.g. Wagner et al., 2004), while noise of the measured spectra hardly contributes to the observed variations. A detailed discussion of the effect of clouds on the Ring effect is provided in Sect. 3.3.’

We also replaced Fig. A3 in the Appendix, by the new Fig. A5, which contains correlations analyses of the data shown in Fig. 5 (as also suggested by this reviewer). The new Fig. A5 also contains O₄ dSCDs as proxy for the cloud effects. The comparison of the O₄ dSCDs and the RSP thus provides a direct link between cloud effects and the variation of the strength of the Ring effect. The new Fig. A5 is shown below:

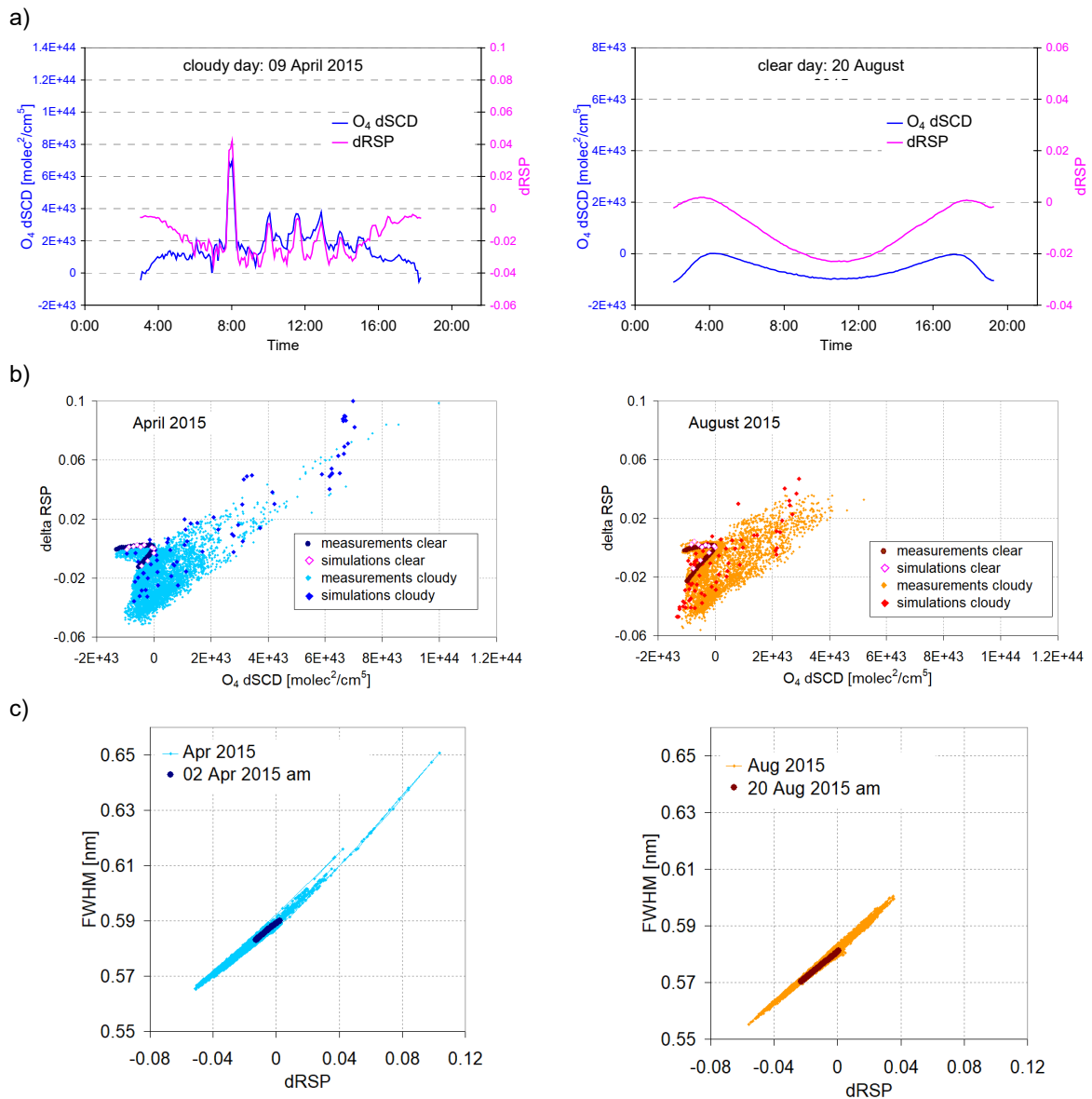


Fig. A5 a) Diurnal variation of the O₄ dSCD and strength of the Ring effect (dRSP) for a clear day (right) and a cloudy day (left). The O₄ dSCDs are analysed in the spectral range 353 – 387 nm. b) Correlation plots of the dRSP and O₄ dSCD for the two selected months shown in Fig. 5. In addition, also results from radiative transfer simulations are shown. The clear sky data cover only a small part of the overall variability, which is dominated by the cloudy cases. For the radiative simulations, clouds with altitudes between 1 and 10 km and optical depths between 1 and 50 were assumed. c) Correlation plots of the FWHM derived from the KF and the dRSP derived from the DOAS analysis for the two selected months shown in Fig. 5.

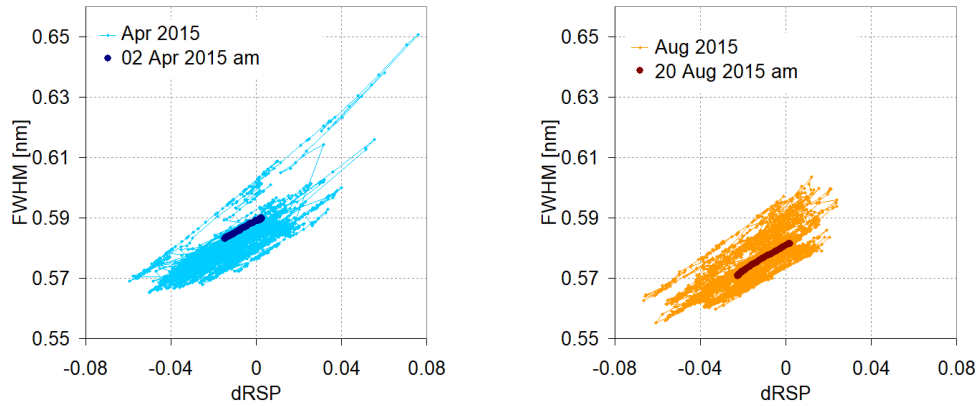
The following text is added (directly before Fig. 5):

‘The influence of clouds was further investigated as illustrated in Fig. A5 in the Appendix. In Fig. A5a the diurnal variation of the strength of the Ring effect and the O₄ dSCD for two selected days is shown. For the clear day, both quantities show a smooth and similar variation. In contrast, for the cloudy day both quantities show strong simultaneous short-term variations caused by light path changes (mainly affecting O₄) and changes of the number and type of atmospheric scattering events (mainly affecting the Ring effect). The different sensitivities to cloud effect also explain why the variation of the two quantities is not always consistent. Fig. A5b

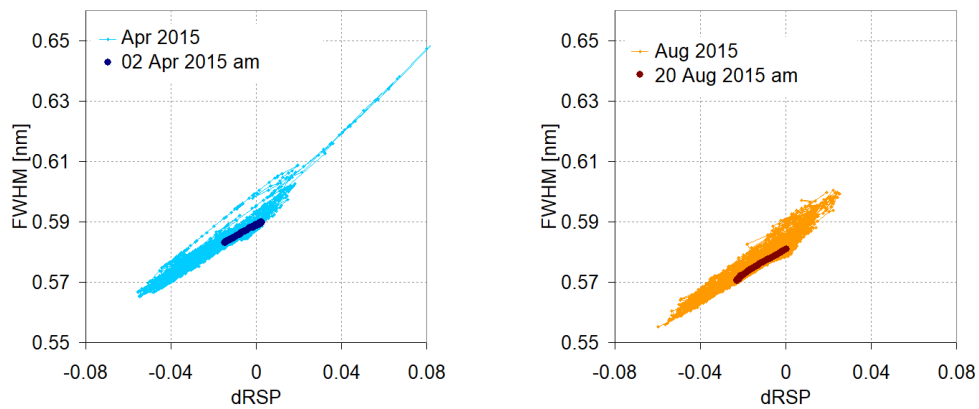
presents correlation analyses of both quantities for all measurements shown in Fig. 5. Also, results of radiative transfer simulations are shown. The comparison of the measurements and simulation results further confirms that the large variations of both quantities are caused by clouds, while the results for a clear day show only rather small variations. Fig. A5c displays the correlation between the FWHM and the strength of the Ring effect for the measurements shown in Fig. 5. The strong correlation between both quantities confirms that the large variability of the FWHM derived for the KF is caused by the effects of clouds.'

Note that the original Fig. 5 was replaced by a new figure with dRSP results obtained using fixed Fraunhofer reference spectra (instead of daily Fraunhofer reference spectra) and excluding the intensity offset in the fit. Fixed Fraunhofer reference spectra were chosen to avoid varying offsets of the retrieved dRSP caused by the changing strength of the Ring effect in the daily Fraunhofer reference spectra. The intensity offset was excluded to minimise the 'cross-talk' between the fitted Ring spectrum and intensity offset. The effect of both changes is illustrated in the figure below. The correlation between the FWHM and dRSP is already present in the correlation analyses of the original data (a), but the results for the individual (half) days show systematic biases along the x axis. If fixed Fraunhofer reference spectra are used, these biases mostly disappear (b), but still some minor variations remain. If also the intensity offset is excluded from the fit, the correlation becomes very tight (c).

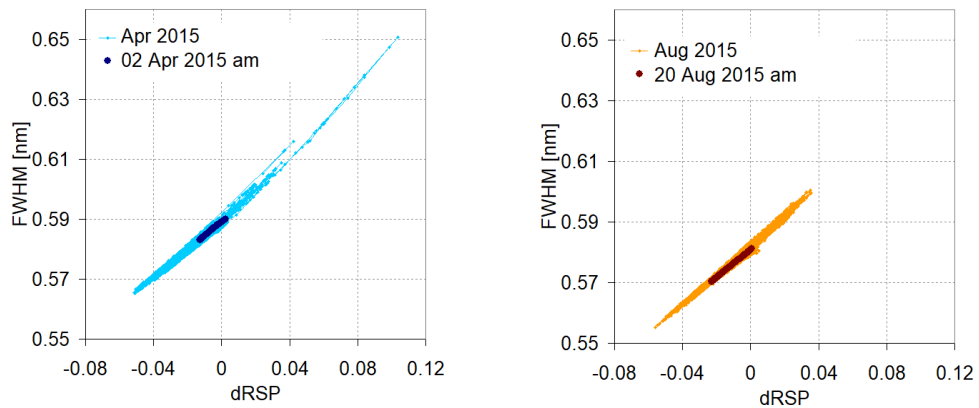
a) Results for daily FRS and with intensity offset (original analysis)



b) Results for fixed FRS and with intensity offset



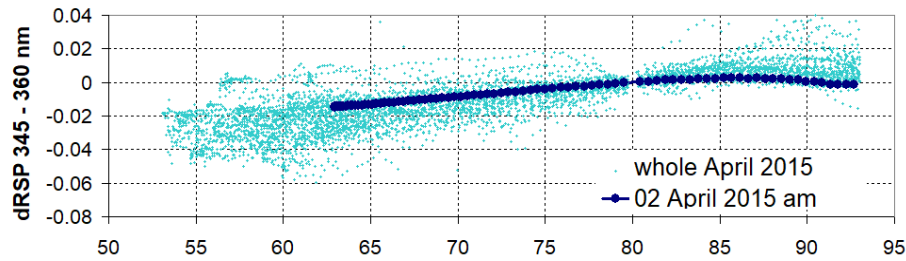
c) Results for fixed FRS and without intensity offset (new analysis)



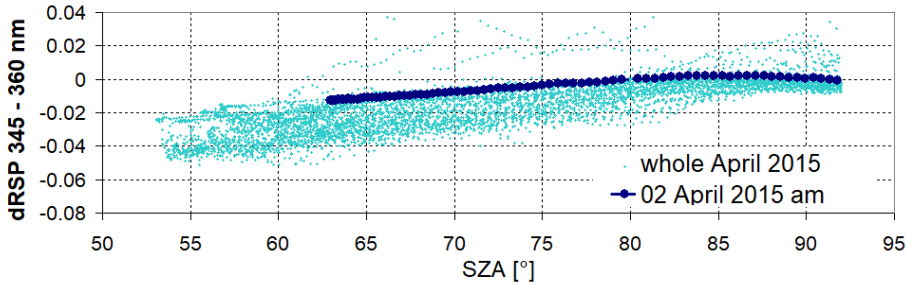
Correlation analyses between the FWHM and dRSP (data from Fig. 5) for different analysis settings.

The comparison of the old and new dRSP results of Fig. 5 is shown below. The main conclusions (SZA-dependence, cloud induced variability) remain unchanged.

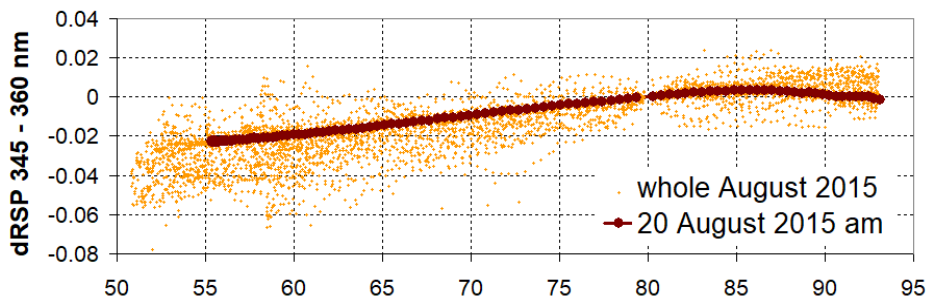
old Fig. 5b



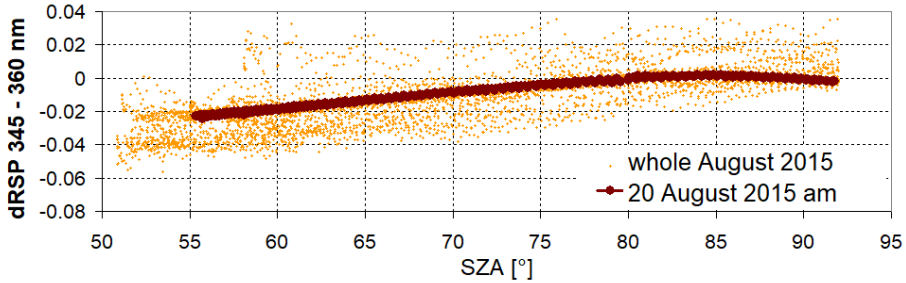
new Fig. 5b



old Fig. 5c



new Fig. 5c



Page 9, lines 205-211 and Page 10, Table 1: Clarify why different fitting parameters are used for each of the BrO bands.

The following information was added to the caption of Table 1: 'For the different fit ranges, different fit settings are used, mainly to account for the effects of the increasing ozone absorption towards shorter wavelengths. For the smallest fit range, a lower polynomial degree and only one Ring spectrum was used, because of the weak effect of the wavelength dependence of the atmospheric scattering processes.'

Page 12, paragraphs 1 and 2: Could correlation plots be added to strengthen the conclusions regarding the various hypotheses?

Correlation plots for the data of Fig. 5 were added as new Fig. A5 as outlined above.

Also, for the data shown in Fig. 4, correlation analyses were added as new Fig. A4 (see below). The following information was added to the text:

‘Correlation analyses of the quantities shown in Fig. 4b-f versus the snow depth (Fig. 4a) are presented in Fig. A4 in the Appendix. The variability caused by clouds dominates the variability of all quantities, but still for all quantities (except for the KF results including an intensity offset) a positive dependence versus the snow depth is found.’

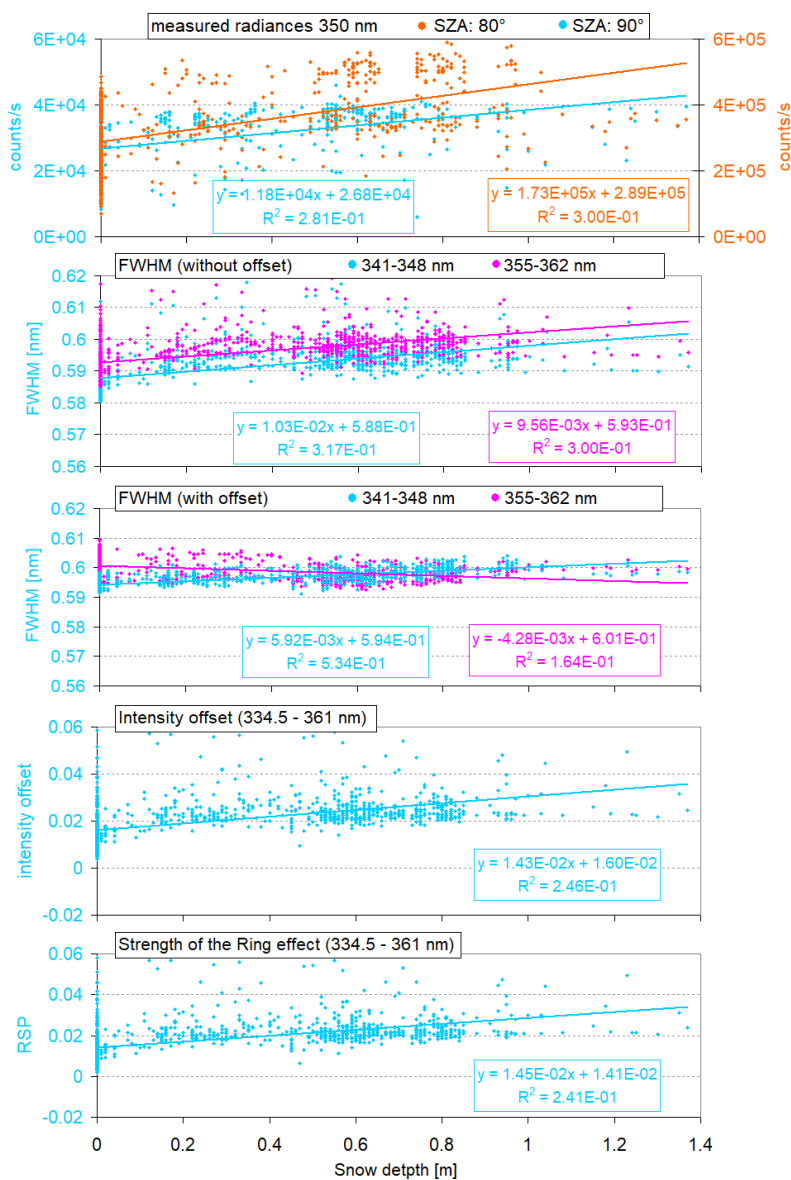


Fig. A4 Correlation plots of the quantities shown in Fig. 4 (b to f) versus the snow depth (Fig. 4a)

Page 28, end of paragraph 2: Comment on relevance of these findings to MAX-DOAS scattered-light measurements.

The last sentence of this paragraph was modified, and MAX-DOAS are now explicitly mentioned: ‘In contrast, for measurements of trace gases located close to the surface, and in particular for MAX-DOAS observations and satellite observations

in nadir geometry, a spectrum without a contribution from Raman scattering, e.g. a direct sun spectrum (or an earth shine spectrum after the subtraction of the Raman scattering contribution), might be a better choice.’

(see also the answer to general comment (4) from reviewer 1)

Page 29, lines 588-591: The reasoning in this last paragraph seems circular (“it is recommended to monitor the instrument characteristics of a DOAS instrument ... In this way ... the instrument properties can be continuously monitored during routine operations”). Suggest rewriting this paragraph for a clearer final message.

We deleted the last sentence.

Technical Corrections

I noted many of the same typographical corrections as Reviewer 1 (ist, Mio, prepared, teh, charateristics, spectromter, etc.) so will not repeat them here.

corrected, see also replies to Reviewer 1

Check the manuscript for correct use of hyphens in compound adjectives before nouns, e.g., for zenith-sky, clear-sky long-term, high-resolution, wavelength-dependent etc.

The use of hyphens was corrected in many places. Further corrections will be applied through the copy-editing process

Many sentences could benefit from the use of commas to separate clauses.

As non-native English speakers, we trust that the use of commas will be optimised through the copy-editing process

Wikipedia is cited twice as a reference (for the length of polar day and night, and for snow cover, at Kiruna). Both should be replaced by references to primary sources.

The reference to wikipedia was removed

Check capitalization of sect./section --> Sect./Section, eq./equation --> Eq./Equation, appendix --> Appendix. Also follow AMT guidelines on when to abbreviate section, equation, and figure.

Numbers could be written as words when not referring to specific values.

corrected according to the AMT guidelines

Add punctuation after equations.

corrected

Page 2, line 32: define DOAS

The definition was added.

Page 2, line 54: Here and throughout - why use “sub window” (sub-window)? No windows have been defined, so why not “fitting window”?

We prefer to use the term ‘sub-window’ because usually the KF is performed in separate sub-windows within the entire wavelength range chosen for the KF.

We added the following information to the text: ‘(in the following referred to as sub-windows, for details see Sect. 3)’

Page 4, lines 92-93, 102, and in legend of Figure 1a: capitalize Swedish Institute for Space Physics

corrected

Page 5, Equation 1: define all terms in this equation
definitions were added

Page 9, line 195: Fig. A4 (not 4A)
corrected

Page 9, line 208: define FRS on first use (it's defined on page 12, line 237)
The definition was added to page 9 and removed from page 12.

Page 10, caption and first row: change Alliwell to Aliwell
corrected

Page 12, line 252: there are no "black dots" in Figure 5 – fix
changed to 'filled markers'

Page 20, line 399: change migh to might
corrected

Page 27, line 519: change monotonous to monotonic
corrected

Page 33, line 698: change self-built to custom-built
corrected

Page 33, line 702: change photo diode to photodiode
corrected

Page 33, line 707: Is the integration 6 minutes at all SZA? Wouldn't noon spectra be saturated for such a long integration time?
Depending on the brightness of the sky, a varying number of scans are taken which are then added to yield the measured spectrum. We changed the text to 'The total integration time (including several scans) for individual spectra (for both detectors) is 6 minutes.'

Page 41, line 800, 805, 806, 811, 825: broad band --> broadband
corrected

Anonymous Referee #3

Author replies are shown in blue
New text is shown in red

Wagner et al. describe methods for characterization of instrumental effects on DOAS retrievals. The analysis uses a novel long-term record (30 years) of spectra from the Kiruna station in northern Sweden. The manuscript is well written and describes important instrumental effects that should be considered for retrievals of trace gas slant columns from ultraviolet absorption measurements. The manuscript fits well into AMT and I would recommend publication. Below are some general comments

on the novelty of key findings, a few topics that the authors should consider, and some small technical fixes.

The manuscript shows that:

The Ring effect (atmospheric Raman scattering filling intensity dips in solar spectra) broadens the instrument spectral response function (ISRF) as measured by fitting of observed solar spectra.

As the Ring effect is enhanced by clouds, the authors suggest cloudy spectra should not be used to determine the instrument spectral response function (ISRF). Authors also show that there is a seasonal variation due to albedo, which enhances ring effect in winter at this high latitude location.

Interestingly, for retrievals of trace gas slant columns of primarily stratospheric absorbers, such as BrO (at this inland location), convolution with the Ring-effect-broadened ISRF appears better because the Raman scattering happens after the absorption by BrO in the stratosphere. However, retrievals lower in the atmosphere may be affected.

The use of convoluted high resolution solar spectra as "Fraunhofer" reference spectra (FRS) is used to develop methods to determine instrumental light throughput and its wavelength dependence, which can improve trace gas retrievals.

The authors make good suggestions for how future researchers can use this information, assisting the DOAS community in consideration of bandwidth effects in DOAS retrievals.

Many thanks for the positive assessment and helpful comments, which we all addressed as outlined below.

Broader points for consideration:

Lines 326-328. In this section, the authors argue that the filling in of the spectral dips by Raman scattering happens after trace gas absorption in the stratosphere and before detection, therefore, the appropriate ISRF is the one including the added width from Raman scattering. This is reasonable, but one point of evidence is that the retrieved BrO summertime values are "unrealistically low". Can there be a citation or more explanation of why the higher summertime BrO values should be accepted?

We changed the text to: 'In contrast, for the analyses using the ISRF from the Raman-corrected spectrum, inconsistent results are found. Moreover, during summer, the BrO dSCDs are found to be very low, not in agreement with model results (e.g. Sinnhuber et al., 2002).'

Also in this section it was not clear if single values of the FWHM used for the full year in the Figure A6 analysis. Can the authors improve clarity on how the ISRF is used in this analysis?

We added the information to the text that the ISRF used for the analyses were derived from the KFs of the spectrum from 06 September 2002. And that these sets of reference spectra were used for the analysis of the whole year.

Line 341. The authors say "varying ISRF gives the more exact results". Do they mean more "accurate" (closer to truth) or more "precise" (smaller variability)?

We replaced 'exact' by 'accurate'

Line 393. The first sentence seems to describe the RSP, but it is not clear. Can "the values" be more clearly defined in this sentence?

'the values' was replaced by 'the values of the fitted intensity offset and the RSP'

About line 515, it says that the nonlinearity from allowing a shift is the cause of errors in the BrO retrieval. Does giving some constraint on the shift suppress this effect? Is the variation in the dSCD correlated to the fitted shift?

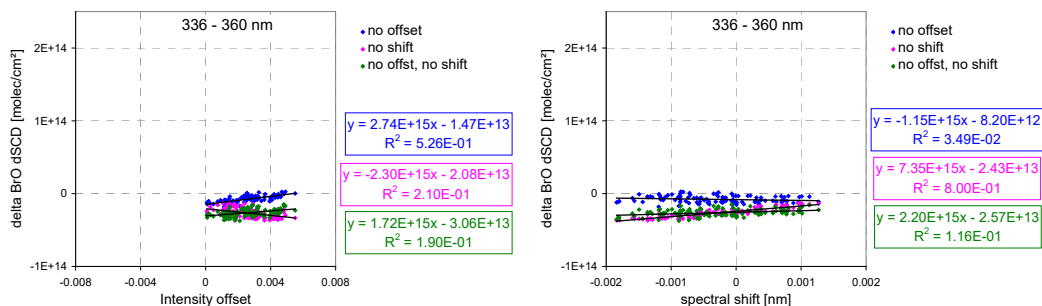
We investigated these dependencies in more detail and added the new Fig. A10 to the Appendix. The following text was added (or original text changed) at the end of Sect. 4.3:

‘The effect of excluding the free spectral shift or intensity offset in the DOAS analysis was further investigated. For that purpose, different DOAS analyses of the original spectra (no etalon correction applied) were performed for two weeks in May 1998. BrO dSCDs were retrieved with either no intensity offset allowed, no shift allowed, or no intensity offset and no shift allowed. The corresponding deviations from the results of the standard analyses (with shift and intensity offset allowed) are plotted versus the shift or intensity offset obtained from the standard analysis. The corresponding results are shown in Fig. A10. Systematically different results are found for the different fit windows indicating that each fit window is affected in a specific way by the respective part of the etalon modulation. The smallest deviations are found for the largest fit window, which contains most spectral information. Except for the smallest fit range, the largest differences from the standard analyses are found if no spectral shift is allowed, and these differences are correlated with the spectral shift. Similarly, if no intensity offset is allowed, the differences are correlated with the intensity offset. The systematic dependence on the spectral shift is similar to the so-called tilt effect (e.g. Rozanov et al., 2011; Lampel et al., 2017, and references therein), but the influence of the Fabry–Pérot etalon effect on the DOAS analyses is more complex. In contrast to the monotonic tilt effect, it causes periodic structures affecting different parts of the fit windows in opposite ways.’

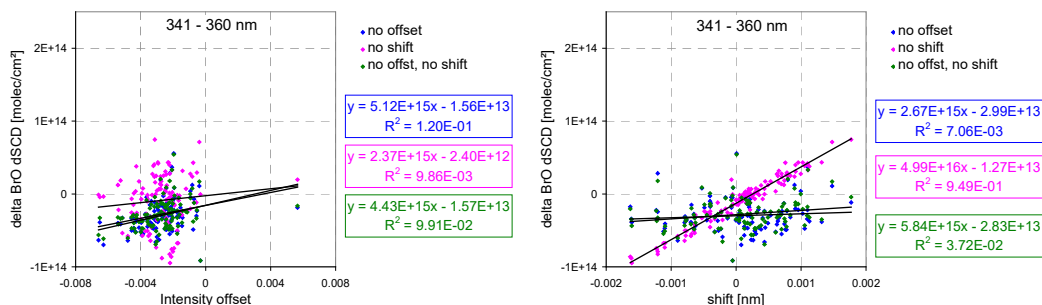
Dependence on the intensity offset

Dependence on the spectral shift

336 – 360 nm



341 – 360 nm



345 – 360 nm

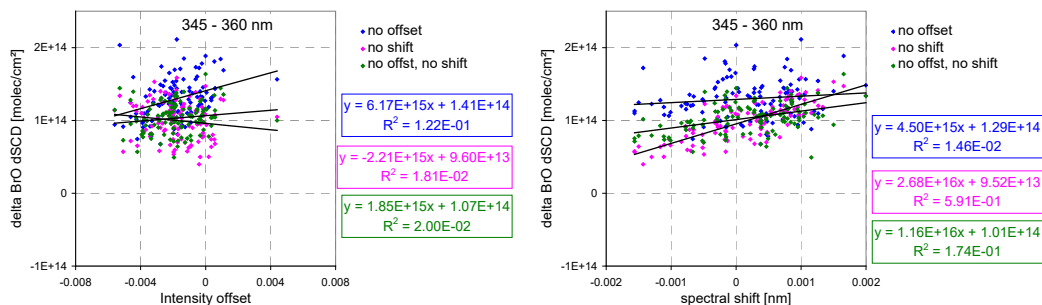


Fig. A10 Correlation plots of the deviations of the BrO dSCDs from those of the standard analyses if the free fitting of a spectral shift, intensity offset, or both is deactivated versus the spectral shift (right) or intensity offset (left) derived from the standard analyses.

The derived shifts are rather small and in the range of typical shifts applied in DOAS analyses (caused e.g. by the tilt effect or instrument effects). But even for such small shifts, the difference to the standard analysis can be rather large. Thus we see no way to constrain the shifts in a meaningful way.

Typographical errors:

Line 61. Replace "ist" with "is"
corrected

Line 87. I'd say "located within the polar vortex"
corrected

p9, line 189. FWHM starts to increase after 2023 -- why is this?

The reason for that change is unclear. Unfortunately, no atomic line lamp measurements during that period are available. We added this information to the text.

During the next visit at the measurement site (hopefully in 2026), atomic line lamp measurements will be performed.

Line 207. Has a typo, which should say "On that day of the year..."
corrected

Line 208. Define FRS.
definition was added

Line 252. I think it should say "a similar monotonic increase of..."
corrected

Line 279. Doesn't the RSP vary with wavelength? The figure cited uses 340nm RSP, which should probably be mentioned. Possibly what matters is weaker wavelength dependence of the Raman / Rayleigh ratio. Can the authors comment?

While addressing this comment, we recognised that the wavelength for the RSP simulation stated in the text and the figure (340 nm) was wrong. The wavelength was replaced by the correct value (350 nm).

To illustrate the wavelength dependence of the RSP (together with its dependence on the surface albedo and aerosol load), we added the new Fig. A6 in the Appendix (see below).

The following text was added to the last part of Sect. 3.3: 'The strength of the Ring effect increases with decreasing wavelength (see Fig. A6). Thus, its effect on the FWHM derived from the KF in the visible spectral range is smaller than for the examples discussed in this study.'

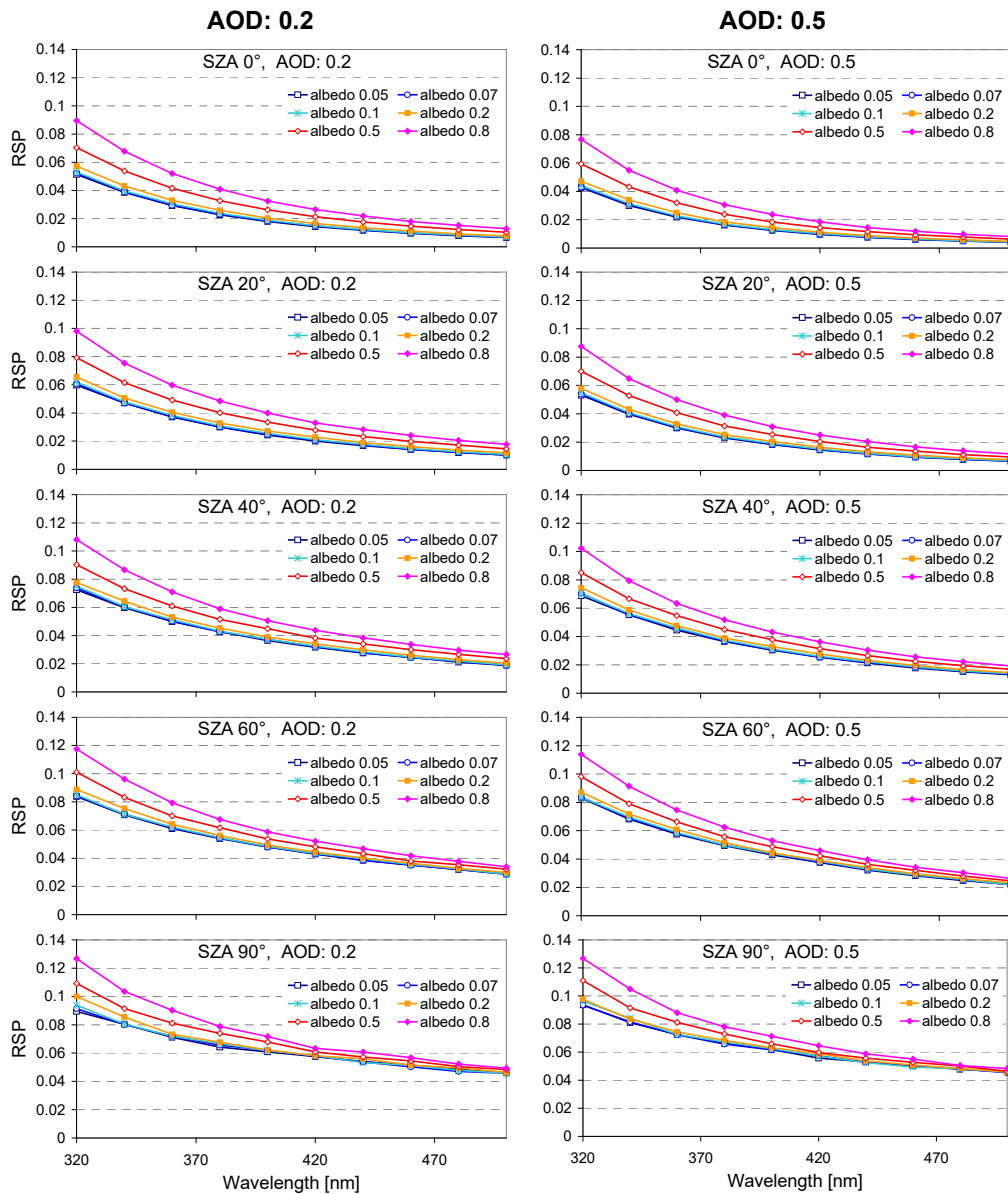


Fig. A7 RSP as function of wavelength for different surface albedos SZA and AOD derived from radiative transfer simulations with the radiative transfer model MCARTIM (Deutschmann et al., 2011). The aerosol profile is assumed as box-profile from the surface to 1 km. Single scattering albedo and asymmetry parameter are set to 0.95 and 0.68, respectively.

Table 2 should mention the unit (nm).
the unit was added

Figure A6 says "with Raman" and "without Raman", but I think "without" is Raman removed, which is a bit different because the Raman was calculated and removal might not be perfectly correct. The x axis should also probably be "date" rather than "time", possibly specifying dd.mm.yy
corrected

Line 399. The word "might" has a missing "t".

corrected

Line 519. Should say "which is monotonic with wavelength..."

corrected