## Reply to reviewer's no.1 comments

We thank reviewer#1 for his/her valuable comments. We tried to address all of them properly. In the following we provide point by point replies to each of the reviewer's comments. Reviewer's comments are in red.

Comments on revised version (see also purple comments in the Replay_RC1_comments.pdf).

**Reply**

We also checked the attached pdf document which helped us to locate where changes should be applied. We thank the reviewer for this document.

The text has been clarified and the figures have been improved.
Most previous comments have been addressed.

Adding Table 2 is useful for comparison of the available UVI operational models.
The revised manuscript can be published with additional technical corrections:

**Reply**

The reviewer should note that considering the proposal of all three reviewers we changed the ETS and the ozone absorption cross sections that were used for the calculation of the LUT in the revised version of the manuscript. All figures, as well as the discussion have been updated properly. As expected, the quantitative results changed only slightly (differences are of the order of 1%) and the main conclusions remain the same.

192: -> dose rates

**Reply**

Done

207: Table 1: explain abbreviations in table caption and wavelength pair for AE
Since the surface pressure and albedo are not included as inputs to the cloudless sky LUT explain how they are accounted for?

**Reply**

The abbreviations have been explained. As was already written in the original version of the manuscript, a surface albedo equal to 0.05 has been assumed for the simulations. In the new version we have also written that the simulations are for the average sea surface level (which, for the considered atmospheric profile, corresponds to pressure = 1013 mb)

217: -> cloudless simulations

**Reply**

Done

218: (mainly O3, SO2, and NO2) – use subscripts

**Reply**

Done

225: Explain which default values were used?

**Reply**

Actually, as it is clarified in the new version of the manuscript, the SO2 and NO2 concentration has been considered to be zero.

230: profiles -> optical properties

**Reply**

Done

231: indicate wavelengths in the Table caption

**Reply**

As it is now also stated in the manuscript "The AE and SSA values have been assumed to be invariant with wavelength for the simulations.". Thus, it is not necessary to specify wavelength.

244: explain CMF abbreviation

**Reply**

Done

246: correct this sentence as following: Using wavelength dependent CMFUV to simulate UV would be more accurate (Krotkov et al., 2001), but ...

**Reply**

Done

258: the cloudless LUT

**Reply**

The suggested change has been applied.

Table 2: Total ozone: Aura/OMI instrument is at the end of its lifetime. Consider switching TOC inputs to OMPS Nadir Mapper or TROPOMI operational products.

**Reply**

We agree with the reviewer. The OMI TOC has been used only to check if we would detect any difference relative to using other TOC products. Nevertheless, for operational purposes the TOC from TEMIS database is used.

294. Do not need "ahead"

**Reply**

Done

311. parameters are the same

**Reply**

Corrected

350. which denotes -> means

**Reply**

Done

350 formulation -> modeling

**Reply**

Done

361. justify -> explain

**Reply**

Done

369: polluted -> pollution

**Reply**

Corrected

371 used -> input

**Reply**

Done

372 Figure 3, which can be compared with a similar Figure 12 for Toronto location (Krotkov et al 1998).

**Reply**

Relevant discussion has been added to the document (lines 375 – 378, 390 - 396) in the revised version of the manuscript.

383. Figure 3: I think that SSA=0.99 is more representative of the average aerosols at DAVOS - see discussion of Figure 12 in Krotkov et al (1998)

**Reply**

Analysis of the SSA at 440 nm from AERONET revealed values that are lower than 0.95. Given that for most aerosol species the SSA increases with increasing wavelength we believe that 0.9 is more representative for Davos than 0.99. Relevant discussion has been also added to the manuscript.

433. correction -> correcting

**Reply**

Done

459. remove "that" is occluded by clouds

**Reply**

Done

490. should be Table 4

**Reply**

Done

503. inability of the model to predict solar disc occlusion by clouds?

**Reply**

Done

510. usual -> standard

**Reply**

Done

Figure 11. Suggest to remove all text on the left which is unreadable.

**Reply**

We decided not to remove the text from the figure, because (1) it provides information for the type of instruments that were used, and (2) it can be red if someone zooms in the figure.

565 "Capturing the instantaneous impact of clouds using satellite images is challenging" - It is impossible since satellite derived CMF represents spatially averaged cloud transmittance over several kilometers, while ground instrument measures instantaneous irradiance at a given location. Time averaging of the ground measurements can only partly mitigate this difference.

**Reply**

The reviewer comment has been inserted in the manuscript

## Reply to reviewer's no. 3 comments

We thank reviewer#3 for his/her time and his/her comments. We considered carefully all reviewer's comments, and we tried to address them. Analytical replies are provided below. The reviewer's comments are in red.

General comments

I would first like to note that I have not reviewed the original manuscript. My review below refers to the revised version (file egusphere-2024-2964-manuscript-version3.pdf). I have also checked whether the response of the authors to the comments of the two reviewers of the original manuscript were appropriate. Accordingly, I confirm that the authors have modified their manuscript appropriately in response to these comments, with the exception of two issues: their choices of (1) the extraterrestrial spectrum (ETS) and (2) the ozone absorption cross section (OCS) used to build the look-up tables (LUTs) for the calculation of the erythemal irradiance. I agree with the comments of the previous reviewers that these choices were unfortunate (as explained in more detail below). I encourage the authors to recalculated the LUTs and process their data with revised LUTs. After all, this paper is about the advancement of their previous model (UVIOS) to the version described in this manuscript (UVIOS2). Rather than improving this model again in the future, it would be better if the model's obvious shortcomings are corrected now. In the "minor comments" section below, I also point out other issues that should be resolved before the manuscript is published.

**Reply**

Following the recommendation of all three reviewers we re-calculated the LUT. We also tried to address each of the major and minor reviewer's comments.

Major comments

L205: The two reviewers of the original version of the manuscript asked why the "atlas plus modtan" extraterrestrial spectrum (ETS) was used in the calculations of the look-up tables and not the newer "QASUMEFTS" extraterrestrial spectrum. This ETS has an expanded uncertainties of 2% between 310 and 500 nm and 4% at 300 nm according to Gröbner et al. (2017; https://doi.org/10.5194/amt-10-3375-2017.) This is the lowest uncertainty in the UV range of any ETS that I am aware of. Without having read the comments by the other reviewers first, I was also wondering why the "atlas plus modtan" (ETS) was used when reading the revised manuscript. This is particularly puzzling considering that this new ETS was produced by several of the paper's co-authors. In their response letter, the authors pointed out that "using a different ETS would require either recalculation of the LUTs or to run libRadtran directly" and then state: "Using a different ETS might result to differences in the simulated erythemal irradiances, as for example was shown in the study of (Gröbner et al., 2017). Based on the results of the latter study we estimate that the simulated irradiances might differ by up to 5% if a different ETS was used, making the used ETS spectrum a major uncertainty factor in UVIOS II simulations." A 5% difference is rather large and this "uncertainty" could be greatly reduced if the QASUMEFTS spectrum were used, assuming that the uncertainty assessment of this spectrum described by Gröbner et al. (2017) is correct. Hence, in my opinion, the right thing to do would be to recalculated the LUTs with this

spectrum. Changing the ETS in the UVSPEC input configuration is rather trivial and scripts to set up the UVSPEC input files for the parameters used in the LUTs should still be available. So it should not be too burdensome to re-calculated the LUTs.

**Reply**

Following the suggestions of the reviewer we constructed a new LUT using the "QASUMEFTS" extraterrestrial spectrum. All figures and tables have been updated and the whole manuscript has been revised accordingly. Differences are in all cases very small, generally smaller than 1-2%, thus the main conclusions have been practically remained the same.

L210: Similar to my last comment, I am also puzzled why the authors use the Molina and Molina (1986) ozone absorption cross section (OCS). This OCS is dated. Current Dobson and Brewer networks and satellite observations use the Bass and Paur OCS, which is also part of the libRadtran/UVSPEC distribution. This OCS should be used if LUTs are recalculated. (Although I like to point out that the Bass and Paur (1985) OCS can now also be considered dated (Voglmeier et al., 2024; https://amt.copernicus.org/articles/17/2277/2024/amt-17-2277-2024.html), but ozone data stored in the WOUDC database have not been updated yet to the best of my knowledge. Since the OCS by Voglmeier et al. (2024) is likely more accurate than the Bass and Paur OCS, it would also be appropriate to use this spectrum instead of Bass and Paur.)

**Reply**

We decided to use the Bass and Paur ozone cross sections as was also recommended by one of the referees to be consistent with the cross sections that have been used to retrieve total ozone from the Brewer (that is used to calculate the UV index from the LUT). As proved by the comparison between the results for the two different LUTs, changing to different cross sections had minor effect on the simulated UVIs.

Minor comments

L29: I would not consider AODs in the order of 0.2 – 0.4 at 550 nm small. Corresponding AODs could easily be twice as large in the UV-B. Please delete "small".

**Reply**

We agree. We changed the manuscript properly.

L185: QASUME is a well-known instrument. Up to now reading to L185, I had assumed that QASUME and not QASUMEII was used as the reference instrument. If in fact QASUMEII was used, QASUME should be replaced with QASUMEII throughout the manuscript.

**Reply**

Done

L201: Please specify the version of UVSPEC.

**Reply**

The version of libRadtran (v2.0.4) has been specified

L228: The publication by Webb et al. (2011) compares the old and new CIE action spectrum for erythema. It should be specified here what CIE norm was actually used (e.g., CIE 1987 or CIE 1998) for calculating the LUTs.

**Reply**

The CIE 1998 action spectrum has been used as it has been specified in the new version of the manuscript.

L260: What does “UVI.2.4” mean?

**Reply**

It was a typo. It has been corrected.

Figure 3: Title "ratio GB" of the Y-axis should be changed to “simulated / measured UVI”. The information that ground-based data was used as inputs to the simulations only needs to be mentioned in the caption or perhaps somewhere else on the plot.

**Reply**

Figure has been changed as proposed.

L370: Results for SSA=0.99 are even more symmetrical about the mean. So from these results I conclude that SSA=0.99 is the actual value for Davos. Why do the authors think that it is 0.9 instead?

**Reply**

Indeed, someone could assume that since the results for SSA=0.99 are more symmetrical, this SSA value is more appropriate for the simulations. However, analyzing AERONET SSA for the days of the campaign reveals values that are generally below 0.95 (in several cases even below 0.8). Since for most aerosol species the SSA either decreases or is similar relative to the SSA at 440 nm, we estimate that SSA should be closer to 0.9 than 0.99.

Sentence starting in L412: It is not clear from this sentence that UVSPEC results obtained with the LUT plus altitude correction were compared with similar calculations where the altitude (or air pressure) were explicitly specified. The wording should be improved.

**Reply**

The manuscript has been modified as follows:

“We compared the UVI that was simulated with UVSPEC for the conditions of the campaign and for the altitude of the site with the results that were derived using the LUT (see Section

3.1.2), and that were post-corrected for the effect of altitude, and we found differences that were generally smaller than 1%."

L427: Regarding "is much faster": Why is it much faster? It should not matter whether a LUT (once it is prepared) is based on CMFUV or COT.

**Reply**

We mean it is much faster than performing libRadtran simulations for each grid point and each time sep with COT as input. We changed the manuscript to make it clearer.

L450: assessed > assess; "UVI when measured or reanalysis timeseries" > "UVI based either on AOD and TOC measured at Davos or taken from reanalysis data."

**Reply**

The suggested changes have been applied to the manuscript.

L467: "Table 3" is not correctly numbered. It should be Table 4 as Table 3 starts on line 276. Also, please Include a row below the title, indicating that columns 2–5 are statistics based on the absolute UVI value while columns 6–9 indicate relative differences. And finally, change QSUME to QASUMEII.

**Reply**

Done

Figure 11: Please explain red, orange and green datasets. (Only the blue and cyan data are described in the text.)

**Reply**

Relevant information has been provided in the Figure 11 caption.

L513: Regarding: "the role of SSA was found to be equally important" I am really surprised that SSA at Davos is as low as reported in the manuscript. Has this been confirmed with independent measurements?

**Reply**

As already discussed, this has been assumed based on the AERONET measurements (that however were not in the UV).

L526: Regarding: "When AOD is 0.3 – 0.4": Please specify wavelength. In general, when AOD is specified, the wavelength should also be provided. And an AOD of 0.3 – 0.4 (presumably at 500 nm) is not low. These are not pristine conditions.

**Reply**

The phrase “even for low aerosol sites such as Davos” has been changed to “even for high altitude sites where the effect of aerosols is usually considered small”. The AOD wavelength has been specified.

Technical corrections

L260: What does “UVI.2.4” mean?

**Reply**

It was a typo. It has been corrected.

L290, L298, L300: The numbers (1), (2) and (3) should also be added to Table 3 that it is clear what column of the table is described here.

**Reply**

Further specifications have been added in the document.

L339: formulation > calculation

**Reply**

Done

L354: justify > explain

**Reply**

Done

L376: “air masses that transfer polluted aerosols from lower altitudes at Germany.” > “polluted air masses originating from low altitudes over Germany.”

**Reply**

Done

L379–381: “in DOY” > on DOY”, “in these days” > “on these days”
Figure 5: The upper broken blue line is missing in the figure.

**Reply**

Done

L404: “in the ratio,” > “to the ratio,”

**Reply**

Done

L407: Regarding “The SSA has been estimated using Kinne,”: Shouldn’t “SSA” replaced with “ASY” here?

**Reply**

The reviewer is right. We changed “SSA” to “ASY”

L497: “to the campaign.” > “in the campaign.”

**Reply**

Corrected

L507: “when solar disc” > when the solar disc”

**Reply**

Done

L511: “reanalysis AOD and TOC that were” > “reanalyzed AOD and TOC data that were”

**Reply**

Done