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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Reconstruction of erythemal UV-levels for two stations in Austria: a comparison between alpine and urban regions" *by* H. E. Rieder et al.

H. E. Rieder et al.

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H.E. Rieder in behalf of the authors:

This is the response to the comments and suggestions of reviewer 2.

First of all we want to thank the reviewer for the comprehensive review and valuable remarks and suggestions that will help to improve the quality of the revised manuscript.

In the following comments of the reviewer will be marked with R2:

The reviewer pointed out that there is confusion between irradiance and doses through the manuscript. The paper is dealing with reconstructed UV-doses and we will change this in the revised version of our manuscript.

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Specific comments:

R2: In the abstract: there is nothing said about the role of the changes in the ozone content and in the cloud cover on the UV trends. It should be added because, as it is stated in section 6, it is an important conclusion.

According to the reviewer comments additional information will be given on that in the abstract. Restriction is however the limitiation of the length of the abstract to 250 words.

R2: p. 960, line 14: "Most of them were based on daily values.". Of what? Please give details.

Here the authors referred to the input data used in the different studies. This will be clarified in the revised version of the paper.

R2: p. 960, line 18: "Only a few studies ... magnitude of the ... levels". Please clarify.

Lindfors and Vuilleumier (2005) have analyzed the influence of ozone, snow and sunshine duration on the reconstructed UV-levels. They found out that from the 1970s most of the changes in the UV-levels result from changes in ozone. While changes in sunshine duration have attributed more to pre-1980s variation in UV-levels then changes in ozone. For more details please see section 4.2, table 4 and figure 7 in Lindfors and Vuilleumier (2005)

For cloud-free conditions Reuder and Koepke (2005) report in their study on a distinct increase in UV-B and erythemally weighted UV-radiation due to reduction of the stratospheric ozone layer. For UV-A they found almost no effect due to changes in TOC. Under cloudy conditions they found that UV-A shows a slight positive tendency during all summer months at Hohenpeissenberg, while for the same location during the other seasons of the year no or only a weak negative trend could be detected. For Wurzburg only for August a positive trend in UV-A could be found out. For UV-B and erythemally weighted UV the general increase found under cloud-free conditions could also be detected under consideration of clouds. For more details please see section 9, table 1 as

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well as figures 4 and 5 in Reuder and Koepke (2005).

R2: p. 961, line 18: the word "global" in global irradiance (end of the line) must be explained, since "global" could mean either (direct+diffuse) or "covering the solar wavelength range".

Here global irradiance means direct and diffuse radiation. This will be clarified in the revised version of the manuscript.

R2: p. 963, § starting line 17: the authors say that they compare the modeled daily total ozone for Arosa and Vienna (or Sonnblick). Then, they give the value of the mean difference [(model_Vienna - Observation_Arosa)/Obs_Arosa]*100%, the value of the correlation coefficient between modeled values for Vienna and observation at Arosa. They refer to Fig. 1b where x-axis is "Arosa_model". Same thing for Sonnblick and Fig. 1c. This is rather confusing, please clarify. At the end of this §, p. 964, they say that they will use Arosa ozone values for Austria since the differences are smaller than 3%. It would be interesting to show what would be the impact of such a difference on the UV-dose.

This point will be clarified. The sentence "....the value of the correlation coefficient between modeled values for Vienna and observation at Arosa." is misleading, since reference is made to a figure which compares two model simulations. We will perform simulations with the RT model to investigate the effect of the ozone determination uncertainty on the accuracy of the UV simulations.

R2: p. 965, lines 21-22: "C is a seasonal correction factor". Why do the authors need such a factor? How is it determined? We learn only p. 968 how it is estimated (Eq. 9) but without any explanation on the choice of this equation. Finally p. 969 it appears that there are 2 C, one monthly and the other seasonal. Again it should be clarified.

The use of a correction factor and cloud modification factors is widely used within the scientific community (e.g. Kaurola et al. 2000, Reuder and Koepke, 2005). The

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correction factor is calculated using a period of simultaneous measurements of UV and global radiation and modeled clear-sky values of UV and global radiation. The basic idea behind the method using cloud modification factors (CMFs) and a correction factor (C) is that the modeling of clear-sky UV-radiation for a specific site can be done more easily than the modeling with clouds. The correction factor C is used to transfer the ratio of Gobs and Gref (the CMFsol) to a CMF for the UV range of the spectra (CMFuv). The correction factor is calculated using a period of concurrent measurements of UV and global radiation and modeled clear-sky values.

R2: p. 966, lines 17-18: "DISORT needs information on ... aerosol optical depth". I don’t see where this AOT will come from. It is only in the summary p. 975 line 2 that the authors say that it was assumed to be constant, but they don’t give the value. Usually, DISORT needs also the type of aerosols (size or Angstrom exponent, asymmetry parameter), the vertical profiles of aerosols, of ozone, of pressure and temperature, the ozone absorption and the Rayleigh scattering cross-sections. If the authors believe that these parameters are not important they should explain why, otherwise they should add info on them.

We have provided only information on the most important input parameters. In the revised version of the manuscript we will give short information on the other input parameters used too.

R2: The sections 4.3 to 4.6 are very confusing and need a big work to become consistent: (i)p. 967: either Eq 4 or Eq 5 must be removed (they are similar), moreover parentheses are missing in Eq 5. What is the difference between Gref (line 12) and Gpot (line 6)?

One of the equations (eq.4) will be removed. It was just added in the manuscript to give reference on the reconstruction method from Neuwirth (1979). Potential irradiance is similar to reference irradiance. Gpot will therefore be replaced by Gref. Text will be changed accordingly.

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R2: (ii) What is the need of Eq 4 & 5 since, as I understand, Eq 6 will be used in the study?

Equation 4 and 5 were only used when no global irradiance data were available. Eq. 4 and 5 were used for the calculation of the global irradiance using the sunshine duration following the Neuwirth (1979) method. Gobs is therefore for our purpose equal to Gmodel. This will be changed accordingly. Equation 6 will be removed since eq. 5 and eq. 6 are similar.

R2: (iii) How is determined X, the correction factor in Eq 6 ?

see former remark

R2: (iv) p. 968, line 3: it is written that the cloud modification factor for UV is derived from UV radiation. Does radiation mean "irradiance" or "dose". It should be specified because in line 19 it is stated that UVmod is for radiation on hourly or on daily resolution and then in p. 969, line 7 (and further), the authors speak of daily doses.

Several time resolutions (hourly and daily) were tested. Here more attention will be paid to the right wordings. The cloud modification factor for UV is derived from hourly UV-doses (HMC and HSC) and daily UV-doses (DMC and DSC).

R2: (v) What is the justification of the expression of the correction factor (Eq 9)? When one reports this value in Eq 1 it comes: UVreko= UVmod * (CMFsol)ˆ2/CMFuv, that is rather strange.

We are sorry for this typo and grateful to the reviewer for addressing it. The right expression of equation 9 is C = CMFuv/CMFsol. We will change that in the revised version of the manuscript This equation follows the Kaurola et al. (2000) approach. That is the way they calculated UVreko in their paper. We just looked on the influence of different temporal resolutions of input data in this equation and how this changes reconstruction quality.

R2: (vi) p. 969, lines 4-5: Two independent data sets are defined but there are not

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described, what are the selected years?

For developing the model, data from the years 1999 and 2002 (Sonnblick) and 2000 and 2005 (Vienna) have been used. For testing the model, data from the years 2000 and 2003 (Sonnblick) and 2003 and 2004 (Vienna) have been used. The combination of these years has been chosen to secure that the same number of days for the comparison is available.

R2: (vii) lines 8-11: the authors conclude that for Sonnblick there is a best fit with the HMCmodel for all seasons. Looking at Table 2, I wonder if the differences between several numbers are statistically significant: for ex. the differences are often equal to 1 (even 0) so it is difficult to conclude that one model is the best. Same thing for Vienna. The conclusion should be reconsidered.

Statistical significance of the results will be addressed in more detail in the conclusion. Regarding the Bias and RMSE the HMC model shows the smallest numbers. Following this it gives the best fit between observations and model results.

R2: (viii) line 13: the content of Tables 3&4 is not detailed, nor the captions (p. 983-984). It should be specified for ex. that in each column is reported the % number of cases within a certain % difference range.

Additional details will be included in the caption according to the reviewer comments.

R2: (ix) line 18: what are the single models?

The single models mean the 4 different reconstruction approaches used in this study.

R2: (x) lines 23-24: "Figures 2 and 3 show the correlation ... UV irradiance". Accordingly, in the x and y-axes it is written "irradiance" but the unit is that of dose and in the captions it is written "dose"! Please clarify.

We are talking about doses here. We will change to dose in the captions.

R2: p. 970, lines 9-10: "UV doses compared with the stratospheric total ozone", the

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total ozone column is not for the stratosphere, moreover it is probably a "yearly average of the total ozone column".

That is right. It is yearly average of total ozone column. Change will be performed in the revised version of the manuscript.

R2: p. 974, lines11-12: the authors state that the "modeling approach using input data ... with the highest temporal resolution show the best fit between estimated and observed UV doses". In p. 969 lines 14-15 it was stated that "the reconstruction quality decreases with increasing temporal resolution". Maybe I have missed something, but it seems inconsistent. Could the authors clarify?

The authors wanted to point out that by using data of larger time steps (daily data) the reconstruction quality decreases. So if increasing time resolution means smaller time step the sentence will be changed to "the reconstruction quality increases with increasing temporal resolution"

R2: p. 975, line 2: It is the first time that the aerosol loading is described (see a previous comment). Lines 3- 7: The calculation uncertainty is "light", it is not really a calculation but rather a comment on what could be the aerosol impact. The authors should consider performing a more serious uncertainty estimation.

Small simulations of possible uncertainties due the aerosols will be made by using realistic aerosol concentrations from literature. R2: p. 975, lines 15-20: I am not convinced with these statements. Ozone concentrations are not at all spatially homogeneous (see p. 964, line 2).

This part of manuscript is rewritten and words "are spatially quite homogeneous" are deleted. In the revised text we explain that the modeled ozone data developed under COST-726 action are used to remove the data gaps. We also add that the hypothesis of using Arosa ozone for the whole Austrian territory was verified, thus there are no speculations about homogeneity of the total ozone field.

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R2: Technical corrections:

Throughout the text, when several references are given for something, the order looks random, neither alphabetical nor chronological.

p. 964, line 23: "Two have" -> "To have".

p. 966, line 20: "(UVMODEL)" -> "(UVMOD)" (to be consistent with Eq 1)

p. 967, line 6: "whereas" -> "where"

p. 969, line 5: "quiet" -> "quite"

p. 982: In Table 2 caption, it should be stated that the BIAS and RMSE values are in %.

Fig. 4&5 show "UV doses" so the y-axes have to be changed, as well as the text in the top of the figures. Same thing for the y-axes in Fig 6&7.

We thank the reviewer for the listing of technical corrections. All technical corrections will be made.

References:

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Neuwirth, F.: Beziehungen zwischen den Monatswerten der Globalstrahlung und der Sonnenscheindauer in Österreich, Arch. Met. Geoph. Biokl., Ser. B, 26, 171–182, 1978.

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