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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Extensive reduction of surface UV radiation since 1750 in world's populated regions" *by* M. M. Kvalevåg et al.

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We thank the reviewers for useful comments to our manuscript. A point by point response is written under each of the referees comment. We have particularly looked at the Method section and improved the description of the set up, instead of referring to several references in order to make it less dense, as both referees suggested.

Anonymous Referee #1 Received and published: 19 May 2009 This manuscript of Kvalevåg et al. investigates the changes in erythemally weighted UV radiation since pre-industrial times caused by anthropogenic changes in several factors (e.g. ozone, aerosol direct effect, aerosol indirect effect, land use, snow cover), by using global CTM. This is an interesting work, focusing also on the other effects than changes in stratospheric ozone. Since this is a modeling study, the relevance of the results is

strongly dependent on the model input related assumptions, for instance how well can one describe the actual surface albedo changes since pre-industrial times. Now this kind of background is written in a very compact way, giving references for further details. However, the reader would be interested in somewhat more detailed description. Examples of these are discussed below. I think the manuscript can be published in ACP after a minor revision, if the comments below are considered.

Method section should be somewhat extended. For instance, now albedo change (both by snow cover change and land use change) is described by two sentences with several references. It would of interest to get a better idea how the snow cover changes affected the albedo used in the modelling. For instance, was the snow depth also included, in addition to snow cover information (since it affects the regional albedo of snow-covered and vegetated surfaces).

RESPONSE: We have extended the Method section to include more detailed description about the set up. This especially concerns the model calculations of surface albedo, snow cover and contrails and aviation induced cirrus clouds.

Text added to the Method section: “The contrail cover from Myhre and Stordal (2001) is updated with the increased aircraft activity reported by Sausen et al. (2005). Information of aviation induced cirrus cover is based on the increase found in Stordal et al. (2005) and related that to the updated contrail cover described above.”

“Snow cover is calculated from the snow depth produced by the two climate simulations. The impact of snow on surface albedo depends on the underlying vegetation types. The albedo in snow covered regions is estimated using the surface albedo for snow-free vegetation, snow-covered vegetation, and snow depth. “ The description of the treatment of the aerosols are also modified; see comment below.

The description of aerosol optical properties could be also clarified. Now in the Table 3, aerosol optical properties are given. Please explain how biomass burning case was distinguished from BC and OC in the model setup, since the two latter ones are also

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components of the biomass burning emissions.

RESPONSE: The following sentence is added to the manuscript: “We assume internally mixing for aerosols from biomass burning and thus OC and BC aerosols from this source have the same optical properties”

Also, you now write (line 7 of block 10462) "... refractive indexes as in ..." This is not the case, since this reference gives the values at visible, while the information in the Table 3 is more suitable for this study. Please check these.

RESPONSE: We have clarified the description of the refractive indexes and also included more information of OC and thus the text is rewritten to

“Sources for refractive indexes and hygroscopic growth information for sulphate, black carbon from fossil fuel, organic carbon from fossil fuel and biomass burning aerosols are shown in Table 3. Aerosol optical properties are calculated with Mie theory based on aerosol size distribution and the source of refractive indexes as in Myhre et al. (2007). We assume pure scattering aerosol for OC from fossil fuel. Measurements of OC show that the absorption in the UV is quite variable from some components having pure scattering to other compounds having rather strong absorption in the UV region (Barnard et al., 2008;Dinar et al., 2007;Kanakidou et al., 2005;Kirchstetter et al., 2004;Martins J. V., 2009;Myhre and Nielsen, 2004;Sun H., 2007).”

Line 29 of block 10464, it seems that there is a mistake in the reference given here for the validation study that included Ispra and Thessaloniki. Maybe Arola et al. 2005 was meant instead of Fioletov et al. 2002, but please check and make sure.

RESPONSE: The reference has been changed to Arola et al. 2005.

Anonymous Referee #2 Received and published: 16 June 2009

In this paper the changes in surface erythemal UV irradiance between pre-industrial times and the year 2000 are estimated using a radiative transfer model and taking into account changes in various components affecting UV, such as ozone, aerosols (direct

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and indirect effects), land use, snow cover, SO2, NO2. Overall this is an interesting paper, well organized and generally clearly written. However, I would recommend that section 3: “Method”, describing the model adjustments for the UV calculations is presented in more detail. My second point is that the significance of the changes in erythemal UV shown in this paper should be presented or discussed in the results section. I recommend publication after processing the comments and suggestions mentioned below.

RESPONSE: We have improved the method section as mentioned in the response to Referee #1. We have also discussed the significance of the changes in both the result section and added a new sentence in the abstract.

1 Introduction (technical comments) Line 5, “We will like” -> “We would like” Line 13, “cirrus”-> “cirrus clouds”

RESPONSE: Done

2. Observed and reconstructed surface UV trends Point out clearly that the trends reported in observational studies are not all for the same period, so that the whole picture is less confusing

RESPONSE: The following sentence is added: “The trends reported in the observational studies shown in Figure 1 cover different time periods with most of the UV measurements starting in the beginning of 1990 and last for less than two decades.”

3. Method As stated in the general comment, I would encourage the authors to elaborate on this section, as it is now rather dense in information. I agree with the comments of Rev. #1 on the snow cover/albedo and aerosol optical properties comments. More specific comments: The use of 5-nm spectral resolution in the UV-B introduces uncertainties in the calculation of UV-irradiance due to the steepness of the ozone cross-section. These uncertainties should be discussed. Furthermore, it is not clear how from the 5nm resolution the erythemally weighted irradiance is calculated.

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Also, the last paragraph of the section (page 10462, lines 24-27) should be placed after the second sentence of the section, i.e. after "...wavelength intervals." (page 10461, line 1).

In this same paragraph the term UV-E as UV erythemal irradiance should be defined before it is used in page 10461 line 20, where it appears for the first time without explanation. Then the sentence 'The model used meteorological data ...' (10461, line 1) could start a new paragraph.

RESPONSE: We have extended the Method section and discuss the issues mentioned above. The UV-E radiation is now defined in the first paragraph of the Method section.

Text added: "The extinction at each wavelength is calculated and integrated into the 5 nm resolution. Test runs show that there is 1% difference between UV irradiances at the surface calculated at each wavelength compared to the 5 nm spectral resolution. We use the CIE-weights which relate UV radiation to human skin damages (McKinlay and Diffey, 1987), thus it is called erythemally UV radiation (UV-E). The action spectrum including the CIE-weights for UV-E radiation is equal to one for wavelengths less than 298 nm and decreases towards zero for longer wavelengths. After the UV irradiance is calculated by the model at the 5 nm resolution, we multiply with the CIE-weight function at each fifth wavelength to obtain the UV-E irradiance. "

4. Model comparisons with observations Here the UV erythemal irradiance calculated by the model is validated against ground based measurements. It would be better to provide further details for the various instruments used, (e.g. type of instrument, location incl. altitude), and the source of the data also in the text and not only in the figure caption. RESPONSE: More information on the observation data is added to this section as follows:

"UV measurements from the ground based instruments were obtained from the World Ozone and Ultraviolet Radiation Data Centre (WOUDC) and the Norwegian institute for Air Research (NILU). Details about the various instruments and sites are available

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in WOUDC (http://www.woudc.org/) except for the data from the Norwegian sites. The sites Oslo, Bergen, and Trondheim are all below 100 m a.s.l. and the instruments at Andøya and Ny-Ålesund are located around 400 m a.s.l. The instrument used at these sites is GUV-511 which is a multichannel filter instrument."

Page 10463, lines 5-6 (and below in line 23): The main (and highest) contributor to this correlation is the seasonal cycle and the ability of the model to reproduce it. It would be nice to discuss a bit further the range of differences, their seasonality, and possibly other factors (except ozone) that could contribute to the differences.

RESPONSE:

Text added: "The reproduced seasonal cycle constitutes a dominating part of the correlation between the observations and the model, but other additional factors are important in order to calculate reasonable UV radiation at the surface. UV absorbing gases and aerosols as well as scattering aerosols contribute to differences between surface UV radiation measured by ground based instruments and UV radiation either retrieved by the satellite or calculated by the model."

In page 10463, line 9 and below: In order to use such a correction, one assumes that the profile differences are constant at all levels at all times. A line explaining this assumption should be added here (or somewhere here) in the text.

RESPONSE: The following sentence is modified as: "A ratio between TOMS ozone and the model is calculated from the total ozone column (in Dobson Unit, DU) and we assume no change in the ozone profiles and only a change in the ozone column. (Eq. (1))."

Page 10464, line 7 and below, also Figure 3. I'm afraid that this figure does not provide more information to the reader, as it could be deduced by comparison between the yellow, blue and red lines of figure 2. RESPONSE: We thank the referee for the suggestion, but believe that this figure and especially the corresponding correlation

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coefficients are valuable information to the reader.

Page 10464, last line: Indeed the reference should be Arola et al., 2005 instead of Fioletov et al 2002 (as reviewer #1 also suggested).

RESPONSE: Changed.

5 Results As mentioned above, it would be nice to show (or even briefly discuss/comment) the significance of the UV changes from pre-industrial times compared to the year 2000.

RESPONSE: We have included the following sentence: “Figure 8 shows a reduction in the surface UV at most land areas over the industrial era, with the largest reduction in many of the most populated regions.”

Additionally, we have added the following sentence as the last line in the abstract: “This reduction in UV-E over the industrial period is particularly large in highly populated regions. “

Also, page 10466, lines 15-16: The reduction in the snow cover affects the winter months at high latitudes, while annual mean UV is dominated by summer. Thus the annual averaging suppresses the larger of the winter months. A line could be added here to make it more clear (or to remind) to the reader.

RESPONSE: Sentence added: “The radiative effect of snow cover change is small because the snow albedo change occurs mostly during winter months when the insolation and exposure of UV radiation is small. The annual average is therefore more dominated by the summer months.”

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