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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Trends in the surface UV radiation at the Polish Polar Station, Hornsund, Svalbard (77°00′ N, 15°33′ E), based on the homogenized time series of broad-band measurements (1996–2016) and reconstructed data (1983–1995)" *by* Janusz W. Krzyścin and Piotr Sobolewski

Anonymous Referee #1

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General comments:

The paper presents a study where measured and reconstructed data of erythemal daily doses in the Arctic station of Hornsund in Svalbard are used to estimate and attribute trends of UV radiation. The reconstructed data were obtained by simple model regressions using data of total ozone and sunshine duration as well as radiative trans-

fer calculations for clear skies. The paper contains useful data and analyses which contribute e to understanding and quantifying the UV variability over the Arctic where measurements are sparse. The quality of the data is not very good and the authors have tried to correct using model calculations. This is not a widely accepted and recommended method and involves great uncertainties, but it can be accepted due to the uniqueness of the location and the scarcity of similar data over the area. In general I miss some estimation and discussion of the uncertainties of the derived results, particularly in estimated derived from the "homogenized" and reconstructed data.

In some parts the discussion is not very clear and should be improved and clarified with some more details.

I miss in the introduction (and possibly on results) section some discussion on reconstruction methods and their uncertainties citing relevant studies appeared in the last 10 years, as for example:

Junk, J. et al.: Reconstruction of daily solar UV irradiation from 1893 to 2002 in Potsdam, Germany, Int J Biometeorol, DOI 10.1007/s00484-00007-00089-00484, 2007.

Lindfors, A. et al.: A method for reconstruction of past UV radiation based on radiative transfer modeling: Applied to four stations in northern Europe, J. Geophys. Res., 112, D23201, doi:23210.21029/22007JD008454, 2007.

Feister, U. et al.: Long-term solar UV radiation reconstructed by ANN modelling with emphasis on spatial characteristics of input data, Atmos. Chem. Phys., 8, 3107, 2008.

Rieder, H. E. et al: Reconstruction of erythemal UV-doses for two stations in Austria: a comparison between alpine and urban regions, Atmos. Chem. Phys., 8, 6309, 2008.

Bilbao, J. et al.: Long-term solar erythemal UV irradiance data reconstruction in Spain using a semiempirical method, J. Geophys. Res., 116, D22211, 10.1029/2011jd015836, 2011.

Lindfors, A., and Vuilleumier, L.: Erythemal UV at Davos (Switzerland), 1926-2003,

estimated using total ozone, sunshine duration, and snow depth, J. Geophys. Res-Atmos., 110, D02104, doi:02110.01029/02004JD005231, 2005.

I consider the title too long: A possible alternative: “Trends in erythemal doses at the Polish Polar Station, Hornsund, Svalbard, based on homogenized measurements (1996-2016) and reconstructed data (1983-1995)”

Although I have tried to mark some of the language errors in the technical comments section, I suggest that the language should be checked again and improved. This will make the paper easier to read.

I believe that the paper can be accepted for publication after sufficient revisions and clarifications of the uncertain parts that are mentioned in the specific comments below:

Specific comments:

1, 27: I would suggest using the term severe ozone loss (or depletion) instead of “ozone hole”.

3, 18: Please check this sentence: “Albedoground =0.9”?

3, 24: Why there is no plot shown for the first period? The correction factors are very large and it would be good to see them in a graph as for the second period, together with the respective standard deviations. Are there any indications in the literature for such rapid deterioration of the sensitivity of RB instruments in five years?

3, 27: How large can be the effect of aerosols at that latitude? This can be estimated with the model for the extreme climatological aerosol data of the Cimel. Then it can be inferred whether aerosols are responsible for the differences, or simply the selection of clear-sky data at high SZAs during spring and autumn months.

4, 2: I find too risky to relay the calculation of trends on data which come from an instrument with such large deterioration. Moreover, for such large year-to-year differences, monthly ACFs would have been more appropriate than yearly.

4, 10: Please mention that by using daily averages for the proxies, it is assumed implicitly that any diurnal variation of erythemal irradiance due to these proxies is not taken into account. Of course this adds to the uncertainty of the estimated daily doses.

4, 17: Please specify where the default aerosol optical depth of 0.16 is coming from.

4, 25-26: If model (3) explains less than half (45%) of the CMF variance then the reconstructed daily doses by (2) should be very uncertain, despite the highly significant regression coefficients. Please elaborate on this in the text, because if the above argument is true, then the results presented later are questionable.

5, 21: I suggest drawing on these figures the linear regressions for the whole period and the observations period with different types of lines, to support the discussion of the linear trends.

6, 2: Please mention whether the negative trends in April-May are statistically significant?

6, 4: Please make clear that by “short period” you mean the period after 1996.

6, 7: It is not clear how the weights were derived and used. Do I understand correctly that the total ozone data and the sun shine duration data were weighted with weights derived from the measured monthly erythemal doses? Moreover, is the yearly dose derived from the 12 months of only from March to September? Please make this section clearer.

6, 10: Isn’t there a circular effect? The data used for the reconstruction were based on TUV calculations which used the measured total ozone, and were adjusted by the CMF which was derived by sunshine duration to account for cloud effects. Therefore, FD_TYD includes already the measured total ozone and the measured sunshine duration, and here it is regressed again against total ozone FD_TO3 and the sunshine duration FD_SUN_DUR. Please explain and clarify the discussion if I got it wrongly.

6, 14-15: This statement (full ozone recovery in 2016) is a bit strong, as the data

presented are weighted averages of ozone. As it is not clear (see previous comment) how the weights are derived and applied, this should be written more carefully.

7, 7: Please mention the statistical significance of the linear trends.

17, 6: Please mention the type of filter used for the smoothing.

Technical comments:

4, 5-6: replace “a cloud cover” by “the cloud cover” and “a sunshine” by “the sunshine”

5, 3: Replace “models (2)” with “model (2)” (singular)

5, 25-27: replace “the trendless” by “a trendless”, “the decrease/increase” by “a decrease/increase” and “the turning point” with “a turning point”

5, 30: Replace “The” with “A”

6, 1-2: Delete “The” (Negative trends $\sim$1% . . .)

6, 13: Replace “provides” with “suggests”

6, 22: Replace “by the instrument sensitivity lost” with “by deterioration of the instrument’s sensitivity”

11, 1: Specify what the bold numbers denote.

14, 10 “monthly doses for the period”; use plural (periods)

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