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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

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Anonymous Referee #2

General comments: The use of sunshine duration and snow depth data for reconstructing long term time series of UV have also been used by other authors, e.g. Lindfors et

al. I miss a more extensive reference list. The calibration constants shown in Figure 2, shows that the instrument used in the period after 2004 has been quite stable, considering the harsh environment. The older instrument shows very high annual drift (factor 2.5 over 5 years). I miss some uncertainty estimates for the measurements series and reconstructed time series. In fact, the instrument deterioration in the period 1996-2001 appeared much smaller about 35% (not 2.5 as it was previously mentioned). We add new Figure (new Fig. 3) showing the loss of instrument sensitivity in this period. We discuss some important studies on the UV reconstruction models (the beginning of section 4). Moreover, a performance of the proposed reconstruction model is compared to the previous study (Lindfors et al., 2003) using similar proxies for UV attenuation in the atmosphere. We explain: "The model setup is almost similar to that used by Lindords et al. (2003) for UV daily doses reconstruction for Sodankylä. However, our model provides RMS error ∼15% for estimates of the daily erythemal dose. Lindords et al. (2003) found RMS error of ∼23%." p.5, 19-21.

A reference to Fig5.b is missing in the paper. Figure 5 has been replaced by Fig.7., which illustrates changes if UV radiation due to combined ozone and albedo effects (simulation by radiative transfer model for clear sky conditions). We think that new Figure illustrates better impact on cloudiness on surface UV at Hornsund.

Figure 5b: If I have understood correctly, the curves in Figure 5b are showing the yearly deviation (residuals) from the mean for the whole period, which means that the two curves labelled "Observed" and "Model" are showing the relative differences in yearly UV doses from their respective means. It would be interesting discussing the differences between real UV observations and modeled UV data. In the revised manuscript, we explain. There are not such curves in new Fig.7. Previous "modeled" curves were obtained with the regression model applied to the weighted data (Eq. 5 in the previous manuscript) that was criticized by the reviewers. The differences between the observed and modeled data (based on cloud modification factor defined by Eq. 2) are shown in new Table 1. p.13

The curve labeled “Observed” is in fact a combination of reconstructed, and measured with gaps complemented with reconstructed UV data, for the whole period 1983-2016. A distinction would be appropriate, e.g. by changing the legend “Observed” to “Combined Observed and Modelled”, or adding a curve with UV observations alone. We decide to delete previous Fig.5 as it combined results of two regression models (previous Eq.2 and Eq.5) and it was difficult to find out meaning of the modeled data. Moreover, the second model was not correctly defined and it was rejected.

Otherwise, one may think the two curves were completely independent on each other. It would also be informative for a reader to see the fractions of the monthly or yearly doses that actually were based on measurements (and not substituted with modelled data). The monthly and yearly doses in the 1996-2001 and 2005-2015 periods are derived from almost every day UV measurements, so the gap existed only for period March 2002 up to April 2005. In the revised paper we calculated trends for both the 1996-2016 time series with the data holes filled by the modeled data and for the time series comprising only observations.

Furthermore (figure 5b), it appears strange that the two curves labelled “Observed” and “Model” are distinctively different for periods where UV observation are missing (1983-1995 and 2002-2004), considering that both are modelled, taking the same input parameters. An explanation would be helpful for the reader. The both curves were modeled by different models; previous Eq. 2 for cloud modification factor and previous Eq.5 for yearly sum of daily doses variability. We do not follow this concept in the revised paper. We explain the long-term cloud effects on surface UV in much simpler way.

Minor comments: Page 1, line 27: “The ozone hole over the Arctic was observed only once in 2011” Even though the ozone layer was record low in the Arctic in 2011, large negative anomalies in total ozone has happened before and after 2011, e.g. in winter 2016/17, see e.g. “State of the Climate 2016”, section J: page S151-S154. http://www.ametsoc.net/sotc2016/Ch05_Arctic.pdf. Please, consider a reformulation.

We add a statement according the reviewer’s comment. “However, severe ozone losses appeared occasionally over the Arctic, e.g. in 2011 (Garcia, 2011; Bernhard et al., 2013) and in 2016 (http://www.ametsoc.net/sotc2016/Ch05_Arctic.pdf).” p.1, l.25-28.

Page 2, line 29-31: “During the two years of its operation . . .”. A reader may first believe the instrument was operating only for two years. The meaning is likely rather “During 2006 and 2007 the instrument was calibrated. We change the text according the reviewer’s comment. p.2, l 28-29.

Page 3 line 4: “Biometer” is normally associated with another brand of erythemal UV radiometers; the Solar Light Co. UV-Biometer. Please, consider using the wording UV-radiometer instead, for all instances of “biometer”. “Biometer” has been replaced by “UV-radiometer” in the revised manuscript.

Page 3 line 18, There should likely be a comma instead of a dot (.) after “>32 cm”. OK. It has been removed.

Section 5 Results and section 6 Discussion and Conclusion: Please, consider restructuring, or moving overlapping information. Example: page 6 lines 1-5 is restated on page 7 lines 4-9. In the revised paper in section 7 (Discussion and Conclusion) we state that “The linear trend calculation by a standard least-squares fit applied to the measured (1996-2016 with the 2002-2004 gap) data shows statistically significant declining tendency in the monthly mean of daily doses (May and June), and in the yearly sum of the erythemal doses. However, such declining tendency are forced by two-three years of high positive fractional deviations of the erythemal doses around 2000.” p 9, l.7-9. According reviewer’s suggestion in this scection, we cut details of the trend (overlapping information) but focus on a source of such trend behavior.

Information on page 6-13 could be moved to the materials section. This part of text has been deleted as it concerns the models’ results not used in the revised manuscript.

Information on page 6 lines 17-24 could be moved to the Discussions section. OK. Now this part appears in the Discussion section.

Page 7 line 14: Belsk should probably be Hornsund. We change the text according the reviewer's comment.

Page 14, legend to Figure 3d: "Monthly doses" are probably monthly mean daily doses. We change the text according the reviewer's comment.

Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2017-619/acp-2017-619-AC2-supplement.pdf

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