

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 #1 General comments: 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 the revised manuscript, the uncer-

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tainty of the daily erythemal doses used in trend calculations have been calculated for the data categories: reconstructed (1983-1995, 2002-2004), measured by a prototype of SL biometer (1996-2001), and KZ biometer (2005-2016). See p.5, l.18-21; p.6-7, l.19-l.7 and Table 1.

In some parts the discussion is not very clear and should be improved and clarified with some more details. More detailed description of the trend methodology and data preparation are included in the revised manuscript 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: (list of publications) This part is added to the main text (p.4, l.16-26) using the reviewer’s list of most important studies. Moreover, a performance of the proposed reconstruction model is compared to the study using similar proxies for UV attenuation in the atmosphere (p.5, l.19-21).

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)” OK. The title has been changed according the reviewer’s suggestion.

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. We have tried to improve the language, for example the manuscript has been read by a foreign speaker.

Specific comments: 1, 27: I would suggest using the term severe ozone loss (or depletion) instead of “ozone hole”. OK. Change according the reviewer’s suggestion.

3, 18: Please check this sentence: “Albedoground =0.9”? New sentence is “AlbedoGROUND is assumed equal to 0.9 for snow depth larger than 32 cm”

3, 24: Why there is no plot shown for the first period? The correction factors are very

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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? New Figure 3 is added showing the instrument deterioration. In fact, the deterioration appeared much smaller $\sim 35\%$ in the period 1996-2001. The previously mentioned deterioration rate ($\sim 250\%$) was erroneously calculated. WMO report (Instrument to Measure Solar Ultraviolet Radiation Part 2: Broadband Instruments Measuring Erythemally Weighted Solar Irradiance", WMO, Rep. No. 164, 2008) stated that the well maintained broadband instrument could lost its stability maximally up to 5% between yearly intercomparisons. Thus, the loss of about 10% per year after two years of stable behavior (1996-1997) seems possible in a harsh polar environment. p. 4, l.1-6.

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. The extreme aerosols optical depth (AOD) for each month (March-September) are determined from 2.5 and 97.5 percentiles of the daily AOD values in selected month by Cimel measurements (2004-2016). These values are used in radiative model simulations to calculate the daily dose uncertainty due to unknown AOD in period prior Cimel measurements. Uncertainty ($\sim 7\%$) of the annual correction factor ACF for the period 1996-2007 is found. See p.4, l.7-14, and Figure 4.

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 In fact, the year-to-year deterioration appeared smaller that discussed in the previous manuscript. It is around $\sim 9\%$ per year, i.e. 9/12% per month. The yearly ACF is derived using mostly from April-June data (i.e. period with many cloudless days). Thus, the July, August, and September value could be underestimated of $\sim 0.75\%$, 1.5%, and 2.25%, respectively, after application of the proposed

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yearly ACF. The trend calculation for each calendar month (March-September) is not affected by ACF changes within the year. Only the yearly trend could be affected. Taking into account a participation of monthly mean doses for these months in the yearly dose (i.e., 23%, 12%, 4% for July, August, and September, respectively, see new Table 1) it could be estimated that using the yearly ACF would provide less than $\sim 1\%$ underestimation of the yearly dose. Thus, using yearly rate of the instrument deterioration instead of the monthly rate affects only slightly trend estimates of the yearly sums of erythemal daily doses.

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. We discuss the problem in the revised paper: "We have no variability of sunshine duration throughout a day. Using the daily values adds additional uncertainties to modeled values as a duration of clear-sky conditions near local noon is decisive for daily doses." p.5, l. 12-13.

4, 17: Please specify where the default aerosol optical depth of 0.16 is coming from. In the revised paper, we explain that "The same procedure was used for the first period (1996-2001) of the UV monitoring at Hornsund but constant aerosols of AOD at 340 nm equal to 0.16 was assumed. During that period there were no Cimel sunphotometer observations. Thus, for the 1996-2001 calibration, we select AOD value representing the mean AOD value found for the period 2004-2016" p. 3, l.28-30.

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. We agree that the explained variability of the reconstructed data is low. However, the root mean square error of the reconstructed data of $\sim 15\%$ (see Table 1) is comparable with performance of previous reconstruction models (e.g. Lindfors et al., 2003 for Sodankyla, Rieder at

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al., 2008 for Sonnblick). Moreover, the proposed Monte-Carlo procedure to calculate the trend error takes into account the uncertainty of the reconstructed daily doses.

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. New Figure 6 is prepared and lines are drawn for the 1983-2016 and 1996-2016 periods.

6, 2: Please mention whether the negative trends in April-May are statistically significant? In the revised manuscript we have: "The statistically significant decline at 2σ level of about 1% /yr is revealed in May, June, and in the yearly sum for the observed 1996-2016 data (with the 2002-2004 gap)." p.8, l.8-11.

6, 4: Please make clear that by "short period" you mean the period after 1996. It has been defined. See response to the previous (6.2) problem.

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. In the revised paper the ozone effects are discussed using different approach without the above mentioned weighting. The daily doses from radiative model simulations for clear-sky conditions are used to calculate the yearly sum of the daily erythemal doses. The clear-sky data are compared with the original (modeled and measured) to discuss the cloud and ozone/albedo forcing of the UV. The yearly sum of daily doses are taken from March-September data because of small intensity in UV radiation in February and October and polar night between end of October and mid February. p 7. l.22-24.

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,

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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. In the revised paper, the different method is proposed to search for clouds and ozone/albedo impact on UV. The results by radiative transfer model for clear-sky conditions are examined in the period 1983-2016. Regression using FD_TYD as a linear function of FD_TO3 and FD_SUN_DUR has been rejected.

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. Problem of the ozone recovery is not discussed in the revised manuscript. We only say that "The stratospheric ozone changes appear as less important driver of the UV long-term variability in the whole analyzed period. Figure 8 shows the long-term (1979-2016) pattern of the total ozone mean (using SBUV merged data) for the period May-August at Hornsund, Barrow, and Resolute, i.e. in the part of the year with naturally high UV radiation ($\sim 80\%$ of total yearly sum). The ozone forcing on the surface UV at these sites appears weak (within the $\pm 1\%$ range) since 1983 (i.e. at the beginning of the reconstructed data)." p.9, l.16-22.

7, 7: Please mention the statistical significance of the linear trends. We define significance of the trend in the revised paper: "The statistically significant decline at 2σ level of about -1% per year is revealed in May, June, and in the yearly sum for the observed 1996-2016 data (with the 2002-2004 gap). The trend analyses applied to the combined observed (1996-2001 & 2005-2016) and reconstructed data (2002-2004) show statistically significant decline only in May of $\sim -1\%/yr$." p.8, l.8-11.

17, 6: Please mention the type of filter used for the smoothing. We explain in the revised manuscript: "Figure 8. Smoothed time series (by LOWES smoother, Cleveland, 1979) of annual fractional deviations." p.22

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Technical comments: The reviewer suggestions (below) are included in the revised manuscript” 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 +/-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)

Revised manuscript is in the attached supplement file. Please note the change of the manuscript title according to the referee #1 comment.

Please also note the supplement to this comment:

<https://www.atmos-chem-phys-discuss.net/acp-2017-619/acp-2017-619-AC1-supplement.pdf>

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2017-619>, 2017.