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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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Response to the reviewer's comments - Referee #3

In general, I feel the paper could be strengthened by discussion of the uncertainties in the results. This might require additional calculations that address sensitivities in the

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Discussion paper

derived results to assumptions in the corrections. The basic difference between the previous and revised manuscript is using a special Monte-Carlo procedure of generation hypothetical time series of daily erythemal doses accounting for various uncertainties for different data categories: reconstructed data, measured by the SL prototype, and measured by KZ instrument (see new section 5). The trend values are derived averaging sample of linear slopes derived by a standard least-squares fit applied to each Monte-Carlo time series.

Uncertainty bars would be very beneficial for the trend analysis discussion. The discussion of the approach to homogenize the observed data for 20 years from the high-latitude station is of benefit. The uncertainty bar appear in the revised manuscript (see new Fig. 6, p.20). Moreover, the model-observation differences are shown in new Tab.1. (p.13)

General comments on instrument correction/calibration:

I do not find in the discussion of the Annual Correction Factor, for the 5 year time period from 1996 to 2001, why the ACF value is so large and reaches a factor of 2.5 over five years. Is that a typical degree of instrument degradation for the Robertson-Berger UV meter? I also miss how sensitive the ACF value is to assumed AOD value of 0.16 and to assumption of no dependency on solar zenith angle. New Figure is added showing the instrument deterioration. In fact, the deterioration appeared much smaller ∼35% in the period 1996-2001. The previously mentioned deterioration rate (∼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. 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 (~7%) of the annual correction factor ACF for the period 1996-2007 is found. See Figure 4. p.18.

Additionally, please clarify what is the time period over which an assumed AOD of 0.16 is assumed: is it 1996-2001 (p.3, l.30) or 2004-2014 (p.4, l.16). In the revised manuscript we explain that "... for the 1996-2001 calibration, we select AOD value representing the mean AOD value found for the period 2004-2016" p.3., l 30. We used this value also if there were no observations of AOD because of bad weather. "daily observed aerosol optical depth (AOD) at 340 nm by the collocated Cimel sunphotometer or AOD equal to 0.16, i.e., equal to long-term (2004-2016) monthly means of AOD at 340 nm, for days without CIMEL measurements " p.5, l. 9-10.

I think more discussion of this result and the implication of the degree to which the trend analysis of the long-term record will be subsequently affected by derived ACF factor is required because there is an obvious knee-bone" around 2006 in the erythemal dosage time series in Figures 4 and 5b. A sensitivity analysis to incremental changes in assumed AOD could be performed at the very least to provide some uncertainty around the ACF value. also I do not find if (and how) uncertainty in the ACF is propagated into the coefficients derived from the linear regression analysis. As we mention before, the trend are calculated using a novel trend method accounting for data uncertainties depending on data collection periods: 1983-1995 for reconstructed data, 1996-2001 for the SL prototype, 2002-2004 for the reconstructed data, and since 2005 up to the end of data for KZ data.

An empirical factor, a function of sunshine duration, is applied to account for clouds. Clouds, due to their temporal and spatial variability, and changing optical properties as a function of low (predominantly water) and high (predominantly ice) altitude will be difficult to proxy model well. Previous paper shows that the combined cloud effects on UV could be parameterized using proxies and solar duration appeared one of possible proxies (see introduction to section 4).

I cannot understand how the sunshine duration, as a proxy of clouds, is found to be highly statistically significant (our response: it means that there is strong linear dependence between the proxy and UV radiation, i.e. long sunshine duration corresponds to higher doses and zero/short duration means small UV doses), when this approach is found to explain only 45% of the cloud modification? (our response: it means that other factors are also important i.e., cloud transparency, period of the day with cloudless condition as noon conditions are decisive for the dose value)

What was the criteria that was used to select sun duration as the best regressor for clouds? We add paragraph (introduction to section 4. Data Reconstruction) providing some details of previous papers focusing on UV modeling. The sun duration was among the regressors used to explain UV variability. There were better set of the regressors (global solar radiation, diffusive component of solar radiation, etc.) but only sunshine duration was available at Hornsund. Of course, it provide a large uncertainty of the reconstructed doses but the trends were calculated taking into the data uncertainty.

The correlation coefficient of greater than 0.9 is reported when regressing modeled and measured erythemal doses (Fig 3). I do not find the sigma (uncertainty in the regression best fit line) reported. We add following statement: “Slope by an ordinary least squares least-squares fit is 0.99 $\pm$ 0.02 (1σ), i.e., it also supports a perfect correspondence between measured and modeled daily doses”, P.5, 28-30.

What uncertainty is assumed/applied for the observed daily erythemal dose in the regression? While standard linear regression does not allow for uncertainties in the regressor, a somewhat related approach called Orthogonal distance regression (ODR) does. I find that clarification and additional discussion about the uncertainty in the proxy model regression to derive the cloud modification factor is required. An assessment of the propagation of this uncertainty into trend analysis would be helpful. Perhaps an ODR approach could contribute to an improved understanding of the sensitivity in the derived scaling coefficients to uncertainties in the modeled erythemal dose. In the revised manuscript, trends are estimated using Monte-Carlo approach taking into account uncertainties in the reconstructed (see new Table 1) and measured data (different values for the measurements by the prototype for the period 1996-2001, and KZ instrument for the period 2005-2016) . New section 5 (Monte-Carlo method for trend estimates) explains the methodology used.

If I understand correctly, a second proxy model of total yearly dose of erythemal radiation is derived from a linear regression of the fractional deviation in yearly dose, where the model contribution in this fractional deviation comes from another multiple linear regression proxy model incorporating sunshine duration. I am not aware of "nested" multiple linear regression proxy models in general. Is this a commonly applied approach and are their references that can be cited as examples? I would feel that the uncertainties from the first proxy model would propagate into uncertainties in the second proxy model (and likely not in a linear fashion due to the nonlinear behavior between clouds, ozone, surface albedo and radiation). Some discussion and acknowledgement of the potential pitfalls of this approach would be helpful in the paper. Our response. In the previous manuscript, the second proxy model was used to find out sources of the long-term variability in UV radiation at Hornsund. Now, we propose much simpler approach in the revised manuscript to solve this task. We use simulations by radiative transfer model to find combined effects of total ozone/albedo changes on surface UV. The clear-sky time series is compared to all-sky series to reveal cloud forcing on UV. Thus, parts of manuscript dealing with the performance of second proxy model have been deleted.

General comments on trend analysis: The proxy model (Eq.2) will be sensitive to clouds, as discussed in the paper. An underlying change in cloud fractions, cloud type (altitude, thermodynamic phase) over long time periods will manifest in the observed surface UV but will not be captured by the proxy model. Therefore, I find that ascribing behavior in long-term trends using the described approach somewhat dangerous, in particular given the large amount of uncertainties inherent in the approach for empirical cloud modification. The analysis that the conclusions are drawn from should really contain uncertainty bars to guide the interpretation of the concluding statements regarding trends in ozone and cloudiness. We are aware of difficulties to estimate trends based on data having different sources and thus variable uncertainties. Simple approach (as that in the previous manuscript) of using only one ordinary least-square linear fit to all or parts of data, provides inappropriate estimate of the trend uncertainty. We propose a novel method to deal with the problem. Statistical analysis of the Monte-Carlo trend sample allows to determine the trend significance based on performance of many hypothetical time series having properties of the original time series. Please note a change of the manuscript title according to the referee #1 suggestion.

Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2017-619/acp-2017-619-AC3-supplement.pdf

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