Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2017-619-RC3, 2017

Atmospheric Chemistry and Physics Discussions
Open Access

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 #3

Received and published: 5 September 2017

The paper discusses the calibration of a long-term surface UV record and erythemal dose (measured or modeled) at a high northern latitude site, Hornsbud, in Svalbard. The surface record is obtained from different ground-based instruments that are impacted by different levels of instrument degradation. There are also gaps in the observational record. A model of erythemal dose has been used, in comparison against

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measurements, to derive annual correction factors to bring the components of the instrumental record on to the same scale for long-term studies. Then, from the homogeneous long-term record, trend analysis of surface UV radiation is performed and linear regression analysis is utilized to attribute changes in long-term trends to physical features, such as clouds.

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 derived results to assumptions in the corrections. Uncertainty bars would be very beneficial for the trend analysis discussion. The discussion of the approach to homogenize the observed data for $\sim$ 20 years from the high-latitude station is of benefit.

General comments on instrument correction/calibration:

Attempts to correct a long-term instrumental record for instrument artifacts is a valuable contribution given the sparsity of ground-based UV radiation and erythemal dose measurements, in particular in the Arctic, where high-latitude retrievals of these variables from satellite observations is challenging due to difficulty in separating bright surface from cloud effects. An annual correction factor (ACF), to correct periods of the instrumental record, such that ratios of modeled to measured erythemal daily dose are $\sim$ within +/- 5% (at solar zenith angles $\sim$ 60 to 70 degrees for the time range 2004-2016.

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. 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). 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.

I also do not find if (and how) uncertainty in the ACF is propagated into the coefficients derived from the linear regression analysis.

General comments on proxy model approach for daily erythemal dose: A proxy model is derived to extend modeled (using TUVS model) surface UV for clear sky between 20065 and 2008 to all sky conditions back to 1983. The proxy model is compared against measurement record in 1996-1999 (corrected by ACF) and 2009-2011 (where ACF = unity). The relationship between US and erythemal dose is a function of ozone column, surface albedo, aerosols and clouds. The TUVS model has the first three as inputs from satellite observations, a parameterized albedo model as a function of snow depth, and aerosol observations. 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.

I cannot understand how the sunshine duration, as a proxy of clouds, is found to be highly statistically significant, when this approach is found to explain only 45% of the cloud modification? What was the criteria that was used to select sun duration as the best regressor for clouds? A 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. 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.

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 propagage 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.

General comments on trend analysis: The proxy model (Eqtn 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.

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