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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Comparison of UV climates at Summit, Greenland; Barrow, Alaska and South Pole, Antarctica" *by* G. Bernhard et al.

Anonymous Referee #2

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General comments

The paper contains a first analysis of nearly 3 years of spectral (UV+visible) monitoring data from Summit, Greenland (at 72 N, altitidue 3200 m) and a comparison with data obtained from Barrow Alaska (71 N, altitiude 8 m) and South Pole (90 S, altitude 2841 m). Spectral UV-monitoring data with well maintained and characterized spectroradiometers are sparse, especially in high latitude areas. The paper makes a valuable contribution in adding this Greenland dataset, and provides a first comparative analysis of UV-irradiances at the three sites. These data and analysis are relevant from the perspective of ozone depletion/recovery and climate change effects on UV, as is well described in the paper. The results show that in the longer UV-wavelengths (345 nm), which are not influenced by ozone, the solar zentih angle dependence of the irradiance

at Summit is similar to the South Pole measurements. The differences that were observed are largely explained by the 4-5% difference that come from variations in the Earth-sun distance. This result implies that albedo, clouds and aerosol and altitude effects for those two sites are comparable. The fact that the effects of the variation of earth-sun distances could be identified in these datasets is indirect additional evidence that the irradiance calibration of the two datasets are comparable. A basic, but non-trivial requirement for the direct comparison of measurements performed at such distant locations. In the ozone-influenced part of the spectrum differences do occur between SUM and SPO and are well explained by differences in the seasonal pattern of the thickness in the ozone column. Clouds and in addition differences in the albedo and altitude give lower UVA and UVB readings at Barrow. The paper addresses the relevant issues and the conclusions and interpretations are well substantiated.

In view of the ozone and climate change issue the paper adds valuable UV-datasets for an Arctic high altitude station, and provides a relevant comparison of these data with those from two other polar sites. The concepts, ideas and tools of the paper built upon previous thorough research performed by the same group (and others), but the comparison made is new, and the validity of the data is well demonstrated. I would have liked to see a little more focus on the UVB (erythemally weighted results) and the presentation of relative contributions of the parameters influencing the UV-climate. Also, some more overall “climatological" numbers on time-integrated UV-radiation levels (year-round, seasonal, year to year variability) could be valuable additions to the presented analyses, which focuses on the comparison of the solar zenith angle dependence. “Comparison of UV irradiance measurements at ...." would be a better suited title for the presented analysis. Despite the fact that nearly three years of data is a short period I could live with the presented title if climatological numbers are added, and some additional analysis is done with respect to the overall effects of the major factors influencing the Uv at these sites in relation to the climatological numbers (clouds, albedo, altitude etc). I give some suggestions below.

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In summary:

- the paper adresses relevant scientific questions within the scope of ACP

- new UV-data for Greenland are presented and compared with data from two other polar sites

- conclusions are clear and substantial, al though per haps not surprising

- methods and assumptions are well demonstrated, clearly outlined and/or well referenced

- results support the conclusions adequately, al though the title is per haps not fully covered by the content. Suggestion either add climatological data, or adjust title (see above and specifi comments)

- the paper builds upon previous work, which is properly referenced and well documented. Data are available for other scientists

- proper reference is given to other papers

- abstract is concise and clear (just minor remarks, see below)

Specific comments

Abstract
Lines 8-9
Impacts of clouds – give more climatological numbers here, such as average cloud modification factors for the three sites

Line 11-12
Unclear to which sites the remarks on aerosol effects refer to?

Line 17
Maximum UV-indices for SPO and SUM are given. It should be noted however that 16 years of data records or only 3 (SUM) influence the record high values. Would 95

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percentile of the readings at the lowest SZA not be a better, more stable measure to mention in the abstract and the paper. Why not include Barrow's high value as well? In addition to the highest values also the median values could be of interest.

Page 4952
Line 25-26
Could a summarizing statement be added on the instrument performance in the inter-comparison: spectral irradiance readings within x % in the UV-range from .. to .. for sza 8230;..

Page 4954
Lines 24-29
Indicate the average ratio and the average sd compared to that average ratio. Is this averaging over the full measurement period and all individual data recordings with sza smaller than 85?

Figure 1
The results for 320, 340 and 380 nm look very similar. That gives confidence but it is preferable to also choose a wavelength in the UVB (310 or 305 nm). I could live with an inclusion of a UVB wavelength and less separate values in the UVA and a textual remark on the equivalence at other UVA wavelengths.

Page 4956
Line 5
The comparison of OMI ozone and ozone from the spectral measurements at SUM (1.010 +/- 0.019) is indicated, but unclear is if this sd refers to each individual measured spectrum or to daily averages/medians? Please clarify.

Page 4956 lines 15-30 and first paragraph of 4957
Can temperature related effects be excluded in the seasonal patterns of 2006 and 2007?

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Fig 2
I fail to see that the yearly pattern for 2005 is significantly different from 2006 and 2007. It appears may be there is more scatter at the start of 2005 but the relevant time period in 2005 (between 18 May and 1 August) lacks data so I doubt the significance. Unless further evidence is available I would suggest to shorten this section, or at least make it less certain: 8220;might be in stead of is absent.

Page 4957, Figure 3 panel b
I fail to see the relevance of panel b in Fig 3: What is the argument to plot the ozone versus SZA. No word is said in the text on this plot and it probably reflects predominantly the seasonal pattern. It could be interesting to see how stable the ozone determination method is in relation to the sza, but then it should be plotted differently (relative change of ozone value over the day/sza).

Page 4958 line 23
Clouds at BAR can reduce UV by more than 75% is not very specific. Better to give some median or 95 percentile indication of cloud effects (or both). Same holds for the number mentioned in the abstract.

Page 4961 line 5-7
The overall maximum of the observed UV-index can be dependent on the number of observational years, unless the year to year variability is low. Add average and or median values for the UV indices, and use 95 percentile values, which are less depending on the number of years.

Page 4961 line 8-9
More than 50 % on average is very flexible: 80% or 90 % fit in that and so does 51%. More exact description should be possible.

Page 4962 line 25-26
The peak (at SZA 84 in figure 8 b) is a consequence of the small ozone column observed on this day. I do not understand this sentence, because I thought all days where

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sza 84 is reached are included so not single days, or is this sza reflecting the lowest sza per day ?!! I do understand that again the seasonal dependence of the ozone value somehow is reflected in this plot. See comment on figure 3b.

Pages 4961-4962
I think here some overall statistics and climatological numbers are lacking. Numbers like: year round, monthly and/or seasonal integrated UV-doses (for erythemally weighted UV) for the three sites. Such numbers would in my view be expected when UV-climate is indicated in the title.

In addition I think the paper would further improve if frequency distributions on cloud modification factors would be added (less detailed but comparable to fig 7 in Bernhard etal 2007; see for instance fig 6 in PN den Outer etal JGR vol 110 D02203 doi 10.1029/2004JD004824). Also a simplified version of fig 8 in the Bernhard etal 2007 paper would be a useful addition in the data-analysis to my opinion.

Technical corrections
Page 4951 line 2 Lehmann, 200 should be 2005
Page 4953 line 7 omit “of" from “with a surface albedo of larger than 0.97 year-round"
Page 4959 line 28 3202m versus 6m => 8m on page 4953

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 4949, 2008.