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Atmospheric Measurement Techniques
Discussions

Interactive comment on "Inter-Calibration of nine UV sensing instruments over Antarctica and Greenland since 1980" *by* Clark Weaver et al.

Anonymous Referee #2

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General Comments:

I recommend this paper for publication, pending consideration of the following comments. In general, the paper would benefit from just a few more details.

The figures should be improved significantly by increasing the font size.

Specific Comments:

[Abstract, p. 2, lines 12–13] *Three of these darkening events are explained by boreal*

forest fires using trajectory modeling analysis.
This is true, but this sentence implies that trajectory analysis is a part of this work, which it is not. Suggest removing "using trajectory analysis".

[Sect. 1, p. 3, lines 5–6] *This paper details the first step: the inter- calibration of radiances from the suite of nadir viewing instruments.*
What are the next steps? It would be nice if these were summarized, if even in a single sentence, to provide context.

[Sect. 2, p. 4, lines 5–7] *Rather than calibrate these additional instruments with a radiative transfer model using LER, we use an empirical approach to remove the solar zenith angle dependence on intensity.*
Although Sect. 3 goes on to explain the empirically based inter-calibration, this statement leaves me wondering why this was chosen over radiative transfer modeling. A simple statement here would establish a context for Sect. 3.

[Sect. 3, p. 5, lines 12–14] *One needs to pay particular attention to make sure the θ_0 used is exactly simultaneous with the intensity, since the SBUV instruments have a different θ_0 for each wavelength.*
A bit more explanation is needed here. These are scanning instruments, and as such wavelengths are not measured simultaneously or at precisely the same solar zenith angle. Is this not reflected in the data product files? Why is particular attention required?

[Sect. 3, p. 5, line 19] *. . . of 663 hPa.*
Why was 663 hPa chosen?

[Sect. 3, p. 6, line 3] *... but the δI was still too dependent on θ_0 ...*
What is meant by this? Simply that the slope derived from VLIDORT (as in Figure 1) was too shallow?

[Sect. 3, p. 6, lines 5–6] *... Jaross et al (2008). They account for the snow BRDF which we omit.*
Would you quantify (at least to first order) what impact not accounting for snow BRDF would have on this analysis?

[Sect. 4, p. 6, line 17] *... NOAA-14 low biased compared to our reference (Figure 2).*
Maybe I am struggling with the color scheme in Figure 2, but NOAA-14 does not appear to be biased low to me. It appears to be positive at least half the time. Am I mis-reading the plot?

[Sect. 4, p. 7, line 2] *After adjustment, the biases are negligible (right panel Figure 3a).*
How is "negligible" defined in this context? Yes, the biases have been reduced, but are they now statistically insignificant? I suggest a different word be used or explained more precisely.

Also, this statement seems out of place. It should come *after* the adjustment is described in the following paragraphs. Here, you could lead with a statement about why adjustment is necessary.

[Sect. 4, p. 7, line 4] *To adjust intensities for a specific instrument a multiplicative factor (c_1) is chosen so that ...*
Is the only reason that the additive coefficient, c_0, is not considered because of the

PMT zero-offset bias mentioned on p. 5? Is this adequately justified? If so, it would be worth stating here.

[Sect. 4, p. 8, line 3] *... they are not used in the intercalibration, but are used in the later trend analysis.*
Are "they" the data affected by the grating drive position errors (presumably corrected for in the trend analysis)? If so, please clarify. And why were they not used (I assume to remove any possibility of contamination)? Please clarify this as well.

[Sect. 4, p. 8, lines 9–10] *It is disconcerting that our correction does not bring them in closer alignment.*
If the authors themselves are disconcerted, then I certainly am. Would you please speculate as to why the correction does not improve agreement? What could this mean for the analysis?

[Sect. 4, p. 10, lines 2–3] *... this merged time series is the geophysical contribution.*
It might be more precise to say "this merged time series represents the geophysical contribution".

[Sect. 5, p. 10, lines 17–18] *For easier comparison we have transcribed the data from their Figure 4 onto our Figure 4c.*
It is still a somewhat difficult comparison in Figure 4. It would perhaps be clearer if the merged time series were compared to MODIS in a dedicated plot.

[Sect. 5, p. 11, line 15] *... on those dates (Figure 8).*
I don't see a Figure 8 in the manuscript. Or is this referring to Damoah et al. (2004)?

[Sect. 6, p. 13, lines 7–9] *These calibrated intensities will be used to derive a UV cloud albedo record over the tropics and midlatitudes since 1980.*
Again, how will these be used to derive a UV cloud albedo record?

Typos:

[Abstract, p. 2, line 9] *While the calibrated intensities show negligible long-term trend over Antarctica, . . .*
Add “a” before “negligible”.

[Sect. 1, p. 2, line 19] *. . . deployed a suit of SBUV-2 instruments on board . . .*
“suit” should be “suite”.

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