Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2020-7-AC1, 2020

Atmospheric Measurement Techniques Discussions

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Interactive comment

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

Clark Weaver et al.

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Received and published: 17 August 2020

Thank you for reviewing our UV calibration paper ...

On the fractional deviation delta_I. This variable occurs through the whole paper, but with different meanings: Of a particular measurement of a dark scene in Figure 1, and thereafter as some (summertime) average in Figure 2, but averaged per SZA bin in Figure 3. Different notations would be helpful. The fractional deviation delta_I that is used throughout the paper is always from Equation 1. The only difference is how it is averaged.

Then, the definition of Delta_I. It is in relation to a certain 4-term polynomial (is that 3rd order? If not, which polynomial orders?). I am actually using a 5-degree polynomial (6

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term). I have corrected the text and figures. Thanks for catching this.

Is it a constraint that the polynomial becomes zero at SAZ=90? In P5,L11 that is suggested, but is it enforced? There is no constraint that the polynomial is zero at SZA=90. Added text "Although the polynomial fit is not constrained to have Iobs=0 at a solar zenith angle of 90o, it appears so, consistent with this instrument design (Figure 1). "

I would expect a deviation with respect to the assumed 'truth' (see Figure 1) , so (I_obs - zeta(SZA))/zeta(SZA). I checked my IDL code and the fractional deviation is calculated as (I_obs - zeta(SZA))/zeta(SZA), text and figures have been corrected. Thanks for catching this.

That said, what is the reasoning behind the fractional/relative deviation (as opposed to absolute deviation)? Now measurements near zero reflection are weighted more heavily, and the expression may blow up (especially when having I_obs in the denominator, instead of zeta). We chose this definition because we are ultimately interested in the percentage error in the intensity.

Are low reflectance measurements more important? Note that the curve zeta itself, (P4L12) seems to be fitted by minimizing the absolute deviations (is that the case?) as standard for LS fitting. Yes, the fitted polynomial Figure 1 minimize the absolute deviations.

Further on Figure 1, the cloud (especially of Greenland) seems to have more outliers below than above the polynomial. Why? The outliers below the polynomial fit (especially over Greenland) are from scenes that have absorbing material (dust, black carbon) in the satellite FOV. Most of the scenes, especially over Antarctica, are free of absorbing material and are at the upper limit of their reflectivity. The ice can get darker but can't get any brighter.

Are the coefficients of the polynomial sensitive to these low outliers? To maintain simplicity we included these outliers in the polynomial fit.

The delta_I is, as said, averaged over summertime. Does that mean that the 14/15 points of NOAA16 in Figure 2 are, on average, zero? The average of all the NOAA-16 scenes will be zero but the annual averages shown in Figure 2 may not exactly be zero.

(NOAA-16 Seems the best choice for reference, but in P4L14 and P4L16, the lifitme is either 2001-2014 or 15 years. Both cannot be true.) Should read 14 year, text corrected.

In Figure 6, the delta_I are averaged for each year, w.r.t. the satellites that were available for each year. That means that with only two satellites active (first year), the points are mirrored around zero. This graph which thus includes these mirroring properties in Figure 6 directly leads to the claim of the uncertainty of 0.35%. But this uncertainty should be different for each year, and years with many satellites should be weighted more than years with two satellites (like 1997) (?) At some point we were doing this. Calculating an uncertainty for each year based on the uncertainty of each individual instrument. In the interest of simplicity, we chose not to present this more complicated approach.

b) On adjusting the intensities. Section 4 starts with the claim that NOAA14 is low biased. How can that be seen in Figure 2? The light orange points do not lie below the NoaA16 points, nor do they lie below the y=0 horizontal line. Can you explain how we should interpret the graph, assuming that the claim is correct? Oops, My error. I corrected the text

The strategy of inter-calibration works because at any time two or more instruments temporarily overlap (chaining). Is there some weighting of very early instruments in the process involved? Are there weak parts of the chain? Conversely, is the solution around 2007 (halfway NOAA16) better behaved than elsewhere? There is no explicit weighting of earlier instruments in the slope or uncertainty determination. I would speculate that 1997 (at least over Antarctica) is weakest part chain because of grating drive errors.

Is it assumed or actively prescribed that the constant terms c0 are zero? It is assumed that all instruments were perfectly calibrated (no offsets). That might not be true. It does not automatically follow that, in this exercise, prescribing c0=0 would be necessary. Of course, it can be tried to allow for non-constant c0 in the inter-calibration. It would probably give better results to allow that freedom (lower residuals) , while necessitating some explaining (...) Initially, I allowed c0 to vary but was later told by those with intimate knowledge of the SBUV instruments that a non-zero offset not possible with the instruments photomultiplier tubes.

Is it correct that the difference between Figure 4 w.r.t. Figure 2 is the correction of I with the gain factor in Table 1, following with the re-computation of delta_I? Yes On the remedy of the hysteresis (P9): So the first light observations of Nimbus-7 were removed. But the associated observations of NOAA16 were not removed, so we do now compare (i.e. in the recomputing process to acquire Figure 4) different summertime averages of delta_I? Is that allowed? The last two panels of Figure 5 show the solar zenith angle delta_I relationship for NOAA-16 (dark traces). Neither show increasing delta_I with decreasing SZA that Nimbus-7 shows. This is a sign of hysteresis. In fact none of the other instruments show this feature, they are insensitive to SZA.

c) On discussing the events.

The 1992 (P9L17) reduction: is it not visible for Greenland? Why not? (Aerosol transport?) We don't know for sure but yes, probably differences in Sulfate aerosol transport.

In P9L21, reductions are mentioned. When are they correlated (Greenland/Antarctica), and when not? And why? The darkening events are from regional transport of light-absorbing particles to the FOV of the satellite so we don't expect a coordinated simultaneous response over Greenland and Antarctica

In general, the point you stress here is that the long-term drift is (just) insignificant,

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but the particular events are well observed by the satellites. That seems OK and well explained. On the other hand, you mention the Polashenski (2015) results to be also 0.05 per decade which is simular (P12L4). If it is insignificant, why mention it? (Can you explain the notation -0.05(0.06) in P11L20 ?)

We mention the Polashenski result because it is an entirely different kind of measurement (in situ). I have added +- before 0.06 to clarify that that it is uncertainty

d) On the graphs. More explaining of the graphs in the caption (in order to have more self-explaining graphs) would be helpful (if that is allowed by the journal).

Technical corrections P2L9: show |a| negligible long-term trend? Done P5L19 stokes -> Stokes Done P8L3: 'they' refers to? Done clarified text P9L7: So the correction of Deland et al was not so good after all and by discarding these 9 minutes we got rid of all hysteresis by brute force (?) No, that's not really fair to Matt. There are uncertainties associated with Deland's calibration. My changes are within these uncertainties. He is familiar with my results.

P9L17: multiple means 2 in this case. Three instruments: NOAA-9 -11 and -14 had grating drive errors. P11L15 Figure 8 -> Figure 7. Thank you P16: Might be an idea to extend the table with lifetime (start-end) per instrument. P16: c0 is - except for OMPS. Why? P16: consider setting c1 to 1 (without zeros) for nOAA16. Fixed OMPS and NOAA-16 . âĂČ

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