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 #1**

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General comments ——————— The paper "Inter-calibration of nine UV sensing instruments over Antarctica and Greenland since 1980" describes indeed what it announces while it also describes, and confirms, some episodic reductions caused by natural events in the last decades.

I recommend the paper for publication, the main reason being the importance of having a well-thought long-term satellite date record. That said, the paper is a bit too concise on some particular aspects, but these aspects can easily be improved/elaborated upon.

Specific comments ——————— a) 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

here. 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?). Is it a constraint that the polynomial becomes zero at SAZ=90? In P5,L11 that is suggested, but is it enforced? I would expect a deviation with respect to the assumed 'truth' (see Figure 1) , so ($I_{obs}$ - zeta(SZA))/zeta(SZA). 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). 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.

Further on Figure 1, the cloud (especially of Greenland) seems to have more outliers below than above the polynomial. Why? Are the coefficients of the polynomial sensitive to these low outliers?

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?

(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.)

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) (?)

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?

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?

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 (...)

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?

On the remedy of the hysteresis (P9): So the first light observations of Nimbus-7 were removed. But the asociated 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?

c) On discussing the events.

The 1992 (P9L17) reduction: is it not visible for Greenland? Why not? (Aerosol transport?)

In P9L21, reductions are mentioned. When are they correlated (Greenland/Antarctica), and when not? And why? In general, the point you stress here is that the long-term drift is (just) insignificant, 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 ? )

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?

P5L19 stokes -> Stokes

P8L3: 'they' refers to?

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 (?) P9L17: multiple means 2 in this case.

P11L15 Figure 8 -> Figure 7.

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.