Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2020-7-AC2, 2020

Atmospheric Measurement Techniques Discussions

Open Access

AMTD

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.

clarkjweaver@gmail.com

Received and published: 17 August 2020

The figures should be improved significantly by increasing the font size. Font size has been increased

[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". Modified abstract, thank you [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.âĂÍWhat are the next steps? It would be nice if these were summarized, if even in a single sentence, to provide context. Added text. "The second step retrieves

Printer-friendly version

Discussion paper

a Black-sky cloud albedo (BCA) record from the inter-calibrated intensities (Weaver et al. 2020) and compares the BCA with the Shortwave CERES cloud albedo." [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.âĂÍAlthough 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. I reworked the text. "At first glance the VLIDORT simulation appears to simulate the observations (red trace Figure 1) and we considered using the I to ïĄśïĂăïĄŕ relationship simulated by VLIDORT as a reference (instead of using NOAA-16). But closer examination shows that the slope of the VLIDORT is shallow compared with the observations. The resulting δI would still be slightly dependent on ïĄśïĂăïĄŕ which would complicate the analysis." [Sect. 3, p. 5, lines 12–14] One needs to pay particular attention to make sure the θ0 used is exactly simultaneous with the intensity, since the SBUV instruments have a different θ0 for each wavelength.âĂÍA 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? I removed the sentence. [Sect. 3, p. 5, line 19] ...of 663 hPa. Why was 663 hPa chosen? It is the mean surface pressure for Antarctica [Sect. 3, p. 6, line 3] ...but the δI was still too dependent on θ0 ...âĂÍWhat is meant by this? Simply that the slope derived from VLIDORT (as in Figure 1) was too shallow? Yes, I reworked the text, see above. [Sect. 3, p. 6, lines 5–6] ...Jaross et al (2008). They account for the snow BRDF which we omit.âĂÍWould you quantify (at least to first order) what impact not accounting for snow BRDF would have on this analysis? Added Paragraph. "Another, more sophisticated approach to validate sun-normalized radiances over ice sheets is described in Jaross et al. (2008). They account for snow surface BRDF and off-nadir viewing angles. Nadir 330nm reflectances simulated using their snow BRDF model are 1% less than those assuming a Lambertian surface at ïĄśïĂăïĄŕ =70o; disparities are near zero at ïĄśïĂăïĄŕ =50o.

Our nadir observed δI was not sensitive to solar azimuth angle over Antarctica."

[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? Oops, My error - corrected the text

[Sect. 4, p. 7, line 2] After adjustment, the biases are negligible (right panel Figure 3a).âĂÍHow 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. See below. 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. Yes, good suggestion, Reworked text "The positive bias for NOAA-17 and 18 is consistent at all ïĄśïĂăïĄŕïĂăbins and suggests that a simple adjustment of the intensities might reduce these biases." [Sect. 4, p. 7, line 4] To adjust intensities for a specific instrument a multiplicative factor (c1) is chosen so that . . .âĂÍIs the only reason that the additive coefficient, c0, 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. Initially, my results showed a non zero offset bias. Scientist/Engineers at NASA and SSAI (Science Systems and Applications) have been working for years to improve the calibration of the SBUV radiances to retrieve accurate ozone products; so they are very familiar with the instruments. They told me that radiances from the PMT can't have a non zero offset; rather what I was seeing are non-linearities at low signal levels. [Sect. 4, p. 8, line 3] . . . they are not used in the intercalibration, but are used in the later trend analysis.âĂÍAre "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. Yes, the other review picked this up too; text has been clarified. Thank you

[Sect. 4, p. 8, lines 9–10] It is disconcerting that our correction does not bring them in closer alignment.âĂÍIf 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? Perhaps the estimated grating positions are wrong. I don't know what else to do. Further work needs to be done on this disparity. [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". Yes that's better Thank you [Sect. 5, p. 10, lines 17–18] For easier comparison we have transcribed the data from their Figure 4 onto our Figure 4c.âĂÍIt 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).âĂÍI don't see a Figure 8 in the manuscript. Or is this referring to Damoah et al. (2004)? Should be Figure 7, text has been corrected. [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.âĂÍAgain, how will these be used to derive a UV cloud albedo record? Added addition sentence, see above Typos: [Abstract, p. 2, line 9] While the calibrated intensities show negligible long-term trend over Antarctica, ...âĂÍAdd "a" before "negligible". text has been corrected. [Sect. 1, p. 2, line 19] . . . deployed a suit of SBUV-2 instruments on board . . . "suit" should be "suite". text has been corrected.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2020-7, 2020.