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Atmospheric Measurement Techniques Discussions

Interactive comment on "Temperature dependence of the Brewer global UV measurements" *by* Ilias Fountoulakis et al.

Anonymous Referee #1

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Brewer instruments were invented, designed and built to measure ozone column which is accomplished by means of ratio-metric measurement which to large extent is insensitive to instrument's optical throughput and PMT responsivity. The subsequent modification of the instrument to expand Brewer's application to measure irradiance seemed like a natural step, however was the stability of the instrument ever good enough to warrant it? This paper like several before it shows that w/o temperature corrections a long term monitoring of irradiance with Brewers will not produce data good enough to promote significant scientific discovery and precise RT model verifications. Perhaps it would be useful to provide a brief literature review highlighting the accomplishments (are there any?) of solar irradiance monitoring with Brewers.

To correct the temperature effect the instrument must be characterized. The instru-

ment can be treated like a black box w/o much understanding of how it operates and what is the mechanism of the temperature effect. One would expect that understanding the mechanism of the temperature effect would lead to a better methodology of temperature effect correction. Unfortunately, the characterization of Brewers fell on the shoulders of users who are forced to treat the instrument just as a black box because the manufacturers did not show much interest in solving the problem that in a better world would be their responsibility.

The thermal equilibrium of instrument with its environment does not imply that it is isothermal. Different parts of the instrument will have different temperatures. The question should be posed whether the parametrization of the black box with a single temperature is sufficient. Should it be the PMT's temperature or the ambient temperature? Obviously the best result will be obtained if both temperatures are used in the parametrization. If the two temperatures are correlated single temperature will suffice. However the perfect correlation is not the case because of the heater and heating from internal lamps, electronics and actuators. Plus there will be hysteresis.

In general case the temperature effect for each temperature range (TR1, TR2, TR3) should be modeled with the formula:

(1+AΔt_pmt)(1+BΔt_ambient)(1+aΔw+b(Δw)^2)

where Δw=w-w0 is wavelength increment form the reference wavelength w0.

You have decided to collapse the coefficients A and B into a single one and neglect the Δt_ambient. What is the cost of this approximation we will not know from your data. Authors should be commended for recognizing that the temperature effect is different in different temperature ranges (TR1, TR2 and TR3). In previous studies of Brewers by Cappellani and Kochler (1999), Weatherhead et al (2001), Garane et al. (2006) and Lakkala et al. (2008) this feature was not recognized. Probably it was because some of the studies used the 50 W lamps that heats up the diffuser which ends up masking the temperature effect of the diffuser. Still in Fig. 2 of Cappellani and Kochler (1999) one can discern that the temperature coefficients are not constant through out the full range of temperatures. In Weatherhead et al (2001) data we see that for wavelengths greater than 325nm the temperature coefficients do not change with wavelengths and that all instruments have very similar (shapes) of temperature coefficients as function of wavelength differing only by wavelength independent offsets. This result may suggest that the wavelength dependence came from the nickel sulfate solar blind filter. But authors of the current study do not agree with it, right?

This confirms that the different patterns found between the three TRs are due to the change in the transmissivity of the Teflon diffuser.

This "this confirms" should be backed up with some illustration in this paper. Results of two tests: through diffuser and w/o diffuser.

For the measurements through the window it was found that the change in the response/°C is wavelength dependent for both the single and the double monochromator Brewers, indicating that the dependence of wavelength might not be introduced by the NiSO4 filter used only in the single monochromator Brewers as suggested by Garane et al. (2006).

Again this is speculative. Ylianttila and Schreder (2005) results suggest that Teflon introduces some wavelength dependence. The quantum efficiency of PMT's photocathode also has some temperature dependence that has a spectral component. Still nickel sulfite can't be acquitted from responsibility for wavelength dependence.

Anyway, Weatherhead et al (2001) results are not congruent with the present work.

I presume that each measurement was preceded with mercury scan to correct the wavelength shift due to temperature. It should be stated.

However correcting the wavelength shift at 297nm does not completely correct the wavelength shift at 325nm or 360nm. This wavelength shift is due to (1) translation of slits away from the optical axis, (2) diffraction grating grooves density change and (3)

micrometer screw expansion. The cumulative effect of wavelength change produces an apparent responsivity change, however this is not the true responsivity change as it depends on the spectral shape of the measured signal and it can't be applied to correct signal when measuring the solar spectral irradiance. It will be different for different lamps that have different spectral gradients dI(w)/dw, where I(w) is lamps irradiance. The authors should estimate this effect. BTW, I do not think anybody was concerned with this effect in previous works. This spurious effect due to wavelength shift may account for part of wavelength dependence in the measured effect. Keep in mind that manufacturer's claims on wavelength stability specs can't be trusted. What impact this study will have on Garane et al. (2006) and Lakkala et al. (2008)? The current results are not congruent with the previous results, right? The Fig 5 is offered as a degree of proof that the correction will improve data quality. I have several issues here: (a) Did you force the points for the red curves to be zero at t=25°C? It is too good to be just fortuitous.

(b) Why the "errors bars" in some case for blue (after correction) are wider than for red? This is counterintuitive. BTW, what do the "error bars" represent?

(c) The red curves should be closely approximated by the ratios of correction factors. I looked at the correction factors in the Supplement and looked at Fig. 3 and I do not get the same shapes as the red curves.

In Fig. 4 there is 315nm mentioned in the caption. It must be a mistake. The red colors for 005 and 037 are too similar. Make the vertical scales of panels a and be the same, i.e., say 0.7 units in each case. Frankly I do not understand panels c and d. 1-sigma of what? Do you have enough points to justify talking about statistics? This has to be explained and justified or dropped.

The paper is not easy to read. I had to go back and forth searching for info whether a given Brewer you were discussing is double or single and so on. I think a table with a list of Brewers, types, nickel sulfate yes or no, locations, temperature ranges, and coefficients (from Supplement) would help. If you use the following formula

(1+A∆t_pmt)(1+a∆w+b(∆w)ˆ2)

the meaning and magnitude of coefficients A, a and b would be more easily readable. The coef A gives you general magnitude for w0 and a and b magnitude change with wavelength.

Also a method of measurements should be grouped in one place as apparently different Brewers were measured at different facilities with different equipment.

Fig. 3 shows that in some cases you did not have too many points. Also there are no data for TR1. Actually, what is the justification for having the same slopes for TR1 and TR3? You are paying here a price for ignoring the ambient temperature. I feel uneasy about TR2 width in some cases. If ranges are really due to Teflon they should be similar among instruments. The issue is that the big change of coefficients between TR2 and TR3 can't be explained by data from Ylianttila and Schreder (2005) and the change should occur in narrower range. Also that TR1 and TR3 are the same is not justified by Ylianttila and Schreder (2005).

I do understand that dealing with these instruments is a real pain. I understand that characterizing temperature effects is not an easy task particularly when you have no right equipment and facilities. So I am not surprised that the paper leaves many unanswered questions. Nevertheless I will recommend it being published providing that authors make some effort to fix and explain some issues that are within their reach. I feel sorry for the author they are forced to engage in such unsatisfactory endeavor.

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