Atmospheric Measurement Techniques Discussions

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Comment on amt-2021-162

Anonymous Referee #1

Referee comment on "Moderate spectral resolution solar irradiance measurements, aerosol optical depth, and solar transmission, from 360 to 1070□nm, using the refurbished rotating shadow band spectroradiometer (RSS)" by Joseph J. Michalsky and Peter W. Kiedron, Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2021-162-RC1, 2021

General comments:
The submission "Moderate spectral resolution solar irradiance measurements, aerosol optical depth, and solar transmission from 360 to 1070 nm using the refurbished Rotating Shadowband Spectroradiometer (RSS)" by Michasky and Kiedron briefly describes the third version of the RSS instrument which operated for several years at the ARM SGP central facility. The submission references previously published work for detailed descriptions of the RSS optical configuration and shadowband operation, with the scope of the current work focusing on specific modifications to the 3rd RSS implemented to mitigate issues with the previous version, followed by a discussion of details related to operational processing of the instrument data to yield quantities of interest to atmospheric scientists including hypeerspectral aerosol optical depth and irradiances.

The operational processing largely follows established methods with the exception of the approach used to determine wavelength registration and the interpolation technique introduced to infer calibrations over wavelength regions for which Langley calibrations are not valid, e.g. water vapor, oxygen bands,etc.

Specific comments -
1 Introduction:
Consider adding 1-2 sentences in the third paragraph to very briefly introduce/identify RSS #1, #2, and #3 which would allow you to eliminate the phrase "the first commercial, i.e., second generation, RSS" which I found to be rather awkward.

2 Fundamental Instrument Details:
The organization of the first paragraph could be improved by introducing each element in the order they would appear from the perspective of the light path. So, start with fore-optic (diffuser and band), integrating cavity, exit slit, prisms, detectors. (Where is the shutter? Without re-reading previous papers I can't remember where it is located in this sequence. It should be described in this section, along with the acquisition of darks, TH,

SB1, BK, SB2.)

line 51 notes: FWHM (full width at half maximum) of 0.6 nm near 360 nm and FWHM of 7 nm near 1070 nm.
OK, but how does the pixel spacing vary with wavelength? The pixel spacing and the spectral resolution are distint properties and both may vary with pixel. Suppose pixel A has center at 360 nm. What is the center wavelength of pixel A+1? Similarly, if Pixel Z has center at 1070 nm, what is the center wavelength of pixel Z-1?

3 Operational Details:

Seems as though dark subtraction shouldn't be necessary for direct beam, right? So even while the shutter was intermittently operating you should still have valid direct beam measurements, unless the shutter position was varying throughout the banding measurements.

Probably should include a reference to Mikhail Alexandrov's paper on the FFT technique to identify band issues.

3.1 Wavelength Registration

lines 96-97 >> pixel shifts ... of up to four pixels in either direction were noted
Earlier, you note the the spectral resolution varies from 0.6 nm to 7 nm from one end of the spectrum to the other, but now you're talking about the wavelength shift in terms of "pixels" and without knowing the pixel spacing it is unclear whether four pixels is 2.4 nm, 28 nm, or some other value. Can you estimate the effect in nm?

I definitely like the approach of using the average of noon-time global horizontal spectra (lowest airmass) for wavelength registration, but you haven't convinced me that a wavelength shift (as opposed to a stretch and shift) is adequate. Frequently, wavelength registration incorporates both a stretch (scale factor) and shift (offset). This might be even more important in the case of the RSS where the spectral resolution changes by more than an order of magnitude from short to long wavelength.

3.2 Estimation of Extraterrestrial Response in Strong Terrestrial Absorption Bands

I have significant concerns about this section.

1. Maybe you should include a reference for standard Langley calibration, and perhaps a 1-2 sentence description? The references provided in this section are for the "modified" Langley, but equation (1) subsumes generation of initial Vo values (for "good" Langley

regions).

2. Equation (1) lists Vo' on both sides of the equation. I'm pretty sure that all of the instances on the right-hand side of the equation Vo should replace Vo'.

3. Line 139 "RSS responses R(lambda)" Do you mean "responsivity"?

4. I don't think the responsivity R(L) is correctly described in lines 148-149. Dividing the lamp output in W/m2-nm by the calibration in W/m^2-nm/count would yield a responsivity with units of counts. According to wikipedia (https://en.wikipedia.org/wiki/Responsivity) the responsivity, in the specific case of a photodetector, measures the electrical output per optical input. So, in the case of the RSS the responsivity should be in units of counts/[W/m2-nm].

5. I think equation (1) confuses rather than clarifies the interpolation process. Substituting "m" for the complicated looking fraction in front of $(\lambda - \lambda_1)$ and "b" for the term at the very end of the equation makes it clear that this is nothing other than a linear interpolation from λ_1 to λ_2. But what is being interpolated? Despite appearances, it is not really interpolating terms of Vo. It is really an interpolation in terms of responsivity R. You can see this by simply dividing both sides of Eq(1) by ET and defining Ro = Vo/ET and Ro' = Vo'/ET. Equation (1) then becomes:

$Ro(\lambda)' = R(\lambda) * \{[(Ro(\lambda_2)/R(\lambda_2) - Ro(\lambda_1)/R(\lambda_1))/(\lambda_2-\lambda_1)]*(\lambda-\lambda_1) + Ro(\lambda_1)/R(\lambda_1)\}$

This is much cleaner. The only terms that are left are responsivity and lambda.

Physically interpolation in responsivity this makes sense because the responsivity is a material property of the detector itself, so whether determined from calibrated lamp output or from solar irradiance the quantities Ro(L) and R(L) should agree whenever the Langley calibration is valid. Thus for wavelength regions between λ_1 and λ_2 (within gas absorption bands), one interpolates the shape of the lamp-measured R(λ) between the values of the Langley measured Ro(λ) across that wavelength range. Mathematically interpolation in responsivity rather than Vo is also preferable becuase responsivity will naturally be smoother since solar and atmospheric features are eliminated or reduced. Then, one obtains Vo'(λ) from Vo'(λ) = Ro'(λ)/ET(λ).

4 Solar Transmission Calculations and Examples

Is an effective cosine-correction being applied to the diffuse hemispheric? If so, it might be good to say so somewhere. Relatedly, in truth aren't only two components truly independent? That is, you're computing ghi = dni + dhi, right? Probably this should be mentioned explicitly. And in later figures in overcast conditions, when dni = 0 it is neither surprising nor a measure of validity that dhi and ghi agree so well. It is a necessary consequence of the fact that you're computing ghi as the sum of dni and dhi, correct?

Moving on, I do have a more fundamental issue. I disagree that the dhi and ghi terms are properly termed "transmission" or "transmittances". It is true that dividing the direct normal measured component V(t,λ) by Vo(λ) (or Vo'(λ)) yields the slant-path atmospheric tranmittance T(t,λ), but transmittance when computed in this way is implicitly a ***one-stream*** property. However, the measured diffuse irradiance is not a one-stream quantity. It must be either multiply scattered or due to a different incident ray (*a different stream*) than that used to define the direct beam transmittance. Aeronet normalizes their measurements of narrow FOV radiances by dividing by ET. They refer to these quantities as "normalized radiance", not "transmittance". I think a similar distinction is important in this case. For example, you might consider using the terms normalized diffuse irradiance and normalized global irradiance instead of referring to these as "transmittances". Not only this this terminology more accurate, it also helps explain the seeming conundrum of ghi > 1. When the sky is horizontally homogeneous, the radiation stream for different parts of the sky are essentialy the same, so one naturally expects dni and dhi to behave like a conserved sum with dni + dhi = ghi < 1. However, under broken (inhomogeneous) skies, it is possible for the direct line of sight to the sun to be cloud-free, while bright clouds away from the line of sight scatter sunlight into the diffuse hemispheric component such that dni + dhi = ghi > 1.

I agree with the authors that instances with ghi > 1 are physically possible and I agree with their explanation of how it occurs. I just disagree with the terminology. As a side note, the ratio between the direct and diffuse components is a calibration indepent quantity that has found use aerosol retrievals, but in general these retrievals require horizontally homogeneous conditions. It would seem that the normalized global hemispheric component (which would also be calibration independent) might be useful to identify spatially inhomogeneous conditions.

The authors note that in Figure 9 teh Ca II, H, and K lines appear as small residuals due to imperfect wavelength registration. These features are also apparent in Figures 6 and 7, btw. However, while the scale of images makes it difficult to assess, the authors explanation seems incorrect. Imperfect wavelength registration would yield a "saw-tooth" in the vicinity of the sharp peak with an enhancement on one side and a reduction on the opposite side. Rather, this small residual may point to an issue with the underlying spectrocopy in the ET spectrum, or more likely slight to inaccuracy in the lineshape of the RSS spectrometer.

Figures 9,10,11: Replace y axis label of "transmission" with "normalized by ET". Fix caption to avoid referring to global and diffuse components as "transmission" because they're not. Also, since dni=0, and dni + dhi = ghi, then dhi is equal to ghi, so you can plot either dhi or ghi and elimnate the other two. Then, condense figures 9, 10, & 11 into one figure to better show the similarity on days with water clouds and the contrast to the day with cirrus.

Figure 12&13: Why the question mark in the title for figure 12? Is this an oblique reference to the possible interference at 504 nm? Can you speculate on the departure from a nearly straight Angstrom relationship between 375-450 nm? Why not condense figures 12 & 13 into one figure keeping the log-log and axes limits from figure 13? It looks like both 870 nm and 1020 nm fall outside 0.008 AOD limits.

Technical corrections:
Figure 4: If you're not going to show units on the y-axis, you may as well hide the numbers as well or normalize to unity. But I'd rather see the units. And it would be very interesting to see R [from the calibrated lamp irradiance] and Ro[from Langley Vo tied to ET irradiance] in this same figure.

Figure 5: 1. Put on same scale as figures 3 & 4.
2. Eliminate ET, not necessary or useful. Also ET doesn't have the same units as either Vo or R.
3. Would be much better to plot responsivities instead of Vo. For one thing, it will avoid confusion from Fraunhofer lines. The two responsivities will only differ significantly in shape in WL regions with gas absorption.

Figure 6,7,8: Replace y axis label of "transmission" with "normalized by ET". Fix caption to avoid referring to global and diffuse components as "transmission" because they're not.

Figures 9,10,11: As above. Also, since dni=0, dhi is equal to ghi, so you can plot either dhi or ghi and elimnate the other two. Then, condense figures 9, 10, 11 into one figure to better show the similarity on days with water clouds and contrast to the day with cirrus.

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