Comment on amt-2021-162
Anonymous Referee #2

This paper reports on the latest version of the Rotating Shadowband Spectroradiometer (RSS) which operates at the ARM site in Oklahoma making spectrally resolved measurements of downwelling solar irradiance in the 360 to 1070 nm wavelength range. The instrument has been upgraded to eliminate deposition on the detector array that degraded the radiometric performance. Additionally, the work describes a technique to radiometrically calibrate spectral regions of strong absorption (e.g. oxygen and water vapor) where Langley analysis to determine the top-of-atmosphere irradiance (the so-called V0) is not valid. The paper also reports on the correction of the spectral calibration (registration) with the use of solar atmospheric absorption features. Finally example spectra of: direct normal, global and diffuse horizontal, transmission, aerosol optical depth spectra are presented. I have several comments and suggestions that should be addressed prior to the publication of this work.

Major Comments:
1) The section 3.2 closely follows the technique demonstrated by Kindel et al (Applied Optics, 2001) using a hybrid of Langley channels in the spectral regions where transmission follows Beer's law and a standard lamp to fill in the regions of strong molecular absorption. In that work, a wavelength independent scaling factor was used to account for geometry and bring the calibration coefficients from Langley and standard lamp into agreement. This had the dual purpose of filling in spectral regions where Langley is invalid (as is done in this work) and demonstrating, to level of uncertainty of TOA solar spectra, about 2-3% and at the time of publication, the agreement between the two methods (standard lamp and Langley) for radiometric calibration of solar spectroradiometers. I am surprised, given the similarity of the technique described here that no mention or description of that work is given. It should be.

[The authors apologize for missing this reference: This paragraph was added: "Kindel et al. (2001) describe a technique for determining extraterrestrial (ET) responses over the continuous solar spectrum using spectral regions where Langley analyses are appropriate and lamp calibrations where they are not. The lamp and Langley calibrations where Langley calibrations are valid determine a scale factor applied to the lamp calibrations that is used to estimate ET responses in the spectral regions where Langleys are not appropriate. In this paper we used the method described below because we missed reading this paper."]

2) Langley analysis is usually performed at high altitude sites where relatively pristine atmospheric conditions prevail. The Mauna Loa Observatory is often used in the community for just such calibration experiments. No doubt, this instrument is fixed at the ARM site and the authors are left with using the ARM site for the Langley calibration. This will introduce higher uncertainties. It is impossible to determine what those uncertainties might be from this paper. Ideally, the V0s should be stable over the course of many Langley plots. A stable instrument, combined with good Langley conditions will reproduce the same V0s from Langley plot to Langley plot. To give the reader an understanding of just how good the instrument and the ARM site are for Langley analysis I recommend plotting the V0s with their standard deviations. I worry that the aerosol environment of the ARM site is less than ideal. As the authors point out, creating a valid (modified) Langley plot for water vapor requires that water vapor is stable over the course of the Langley plot measurement time frame. This is equally true of aerosols, although likely not as problematic as water vapor. Additionally, the V0s generally have higher uncertainties in the shorter wavelengths. The higher optical depths likely encountered at the ARM site will generate larger slopes that create larger changes in the V0s when extrapolated to the TOA.

[This paragraph added with references to explain why we think Oklahoma data are okay: "While Langley calibrations are ideally performed at high altitude sites, such as Mauna Loa Observatory (MLO), we have demonstrated that calibrations can be performed at less than this ideal site using a sufficient number of Langleys. Perhaps the best example of this is from Michalsky and LeBaron (2013). Using the same multifilter rotating shadowband radiometer (MFRSR) at MLO and in Boulder, Colorado, we obtained the same calibration in the five aerosol channels of the MFRSR to within 0.006 worst case. Granted, the signal to noise was much better at MLO even with fewer Langley events, but the medians at both places agreed (see Figure 1 in Michalsky and LeBaron (2013)). Note that the MFRSR and the RSS use the same shadowing procedure in every respect, therefore, we expect similar results from either instrument."]

3) The authors carefully identify the source of nearly every absorption feature in the spectra shown. However, there is an effect that is not discussed at all. The so-called planetary problem. This is the result of multiple scattering between the surface and the atmosphere. In measurements of downwelling global spectral irradiance taken from aircraft it's common to see the signature of the vegetation near infrared edge (~690-800 nm) when flying below a cloud deck. Indeed, the first surface measurements made with an ASD-FR spectroradiometer and cosine diffuser showed this effect and it remained a mystery until later explained by Dr. Peter Pilewskie who made similar measurements with the Solar Spectral Flux Radiometer. Thankfully, over the ocean this effect is largely absent thus the strong preference for using flight data over ocean for shortwave cloud studies. I strongly suspect that this type of effect on the spectral shape of the irradiance can be found in RSS data. I understand that it's extremely hard to quantify without good measurements of surface spectral albedo and even then I'm not sure how those measurements would be used to correct the data. The authors should, at the very least, acknowledge the unknown continuum effect that the surface albedo has on the RSS measurements, particularly in the presence of clouds, perhaps showing a plot of a global irradiance spectrum obtained during peak growing season under cloud cover. The feature is generally easy to identify.

[**We thank this referee for reminding us of this effect; this was added: "This increase in transmission, outside molecular bands is likely caused by the increasing surface albedo with wavelength (see, e.g., McFarlane et. al. 2011), which increases the shortwave radiation scattered to the clouds, hence, causing them to appear brighter in the near-infrared."]**

4) The comparison of aerosol optical depths from RSS and the well-established Cimel instrument (Figure 13) are very useful and give confidence to the results obtained by RSS. However, a single comparison, as good as it is, it not useful. Surely, this figure can be expanded so that statistics can be calculated to see how well the two instruments agree. Perhaps aerosol optical depths from both instruments on those days used for the Langley analysis could be included?

[We added references to earlier papers that have shown this comparison for shadow banding instruments that work in exactly the same mode.]

5) There is no summary/conclusions section to this work. I often find this, after the abstract, to be the most important section in any scientific paper.

[Now there is.]

Minor Comments:
1) The use of Kurucz/Gueymard is outdated. The TSIS Hybrid Solar Reference Spectrum has uncertainties substantially below 1% for most of the wavelength range discussed in this work. (Coddington, 2021). I understand that this comes too late for this work, but I would encourage the authors to reference this work as a potential fix for their comment on line 265 about the TOA solar spectrum uncertainty.

[Added text indicating new ET spectrum.]

Additionally, the Kurucz spectrum is scaled by Gueymard. It's not clear how. Is it normalized at a single wavelength or by their integrated values?

[Not sure how to improve statement.]

2) Line 207-208, I'm not sure what is meant by the sentence "The feature labeled H2O(?) is likely a water vapor band that is in the HITRAN (2012) database". The feature is in the spectrum and I assume all the features described in the paper are in the HITRAN database or atlases of solar spectra. Please reword.

[Uncertain wording removed and now stated that it is water vapor as in HITRAN.]

3) Line 213-214. "... is the broad depression in what looks like continuum". Chappuis absorption doesn't look like continuum, it is continuum. Reword.

[Reworded.]

4) Line 122 (Kindel, 2001) used synthetic Langley plots created with MODTRAN to show exactly where in the spectrum(400-2500 nm) Langley (i.e. Beer's Law) is valid.

[This was added.]

5) Line 233, please explain how Rayleigh and Chappuis were removed, it has a large effect on the values of the aerosol optical depth. On line 183, the retrieval of ozone from the Chappuis is "less obvious and more uncertain". Certainly this applies to its removal to determine aerosol optical depth as well?

[References to how it was done were added.]

6) The transmission uncertainty is estimated at 1% on line 265. Following the comments above about the lack of uncertainty analysis in the generation of the V0s it's impossible to assess this claim.

[Reference to how this determined for spectrum not affected by molecular absorption added.]