

This paper reports on the performance a new radiometer, HSR1, that measures total and diffuse spectral irradiance. HSR1 measured irradiances, diffuse ratio, and derived aerosol optical depths are compared to other spectral and broadband radiometer systems: MFRSRs, SASHe, a Cimel sunphotometer, the RadFlux data product. The instruments operated at the ARM Southern Great Plains site for two months in spring/summer of 2022. The conclusion is that HSR1 measurements of total and diffuse spectral irradiance well in relation to the other instruments, however significant biases exist for irradiance measurements near the tails of the instruments spectral range.

This paper does a reasonable job with the statistical comparison between the HSR1 data and the data collected by the reference instruments at the SGP site. However, for a paper that aims to detail the functionality of a new instrument, key components of the manuscript are lacking. These include: details about the HSR1 instrument hardware, calibration procedures, and the study design that are necessary for an instrument paper of this sort. A discussion of the measurement biases in the context of the unique instrument design features. The role measurement uncertainty has on the analysis is not sufficiently addressed. Further, the quality of the writing, both with respect to grammar and structure of the paper, is not at an appropriate level for publication. I've highlighted a few examples of writing quality issues in the following comments. Given these issues I do not recommend this manuscript for publication in AMT at this time.

Comments:

Lines 23-25 – “The HSR1 quantities are also compared at other wavelengths to the collocated instruments, where similar agreement is found for the spectral irradiances, although relatively larger disagreement is found at higher wavelengths, especially for spectral AODs.” To me this sentence reads awkwardly. I recommend that it be reworked.

Line 44 – It is worth pointing out somewhere in the manuscript that HSR1 measures total and diffuse irradiance simultaneously, which is in contrast to rotating shadowband systems.

Line 45-59 – Much of the contents of this paragraph that detail characteristics of HSR1 should be moved out of the introduction to a section that overviews the hardware and calibration procedures of the instrument.

Line 55 – “As the sun moves across the sky throughout the day...” It is also worth mentioning here that the same holds true if the instrument position moves. Again, this seems to be a unique feature of the shadowmask design of the HSR1.

Line 57-59 – “The measured diffuse assumes that the diffuse light is scattered equally angular, i.e., isotropic. The isotropic assumption may not be applicable due to the scattering properties of aerosols and clouds which may have a preferential scattering angle.” Here is an example of where the writing quality needs to be improved. This paragraph would read more clearly if the writing were more concise, e.g.: The measurement of diffuse irradiance assumes the scattered

light is isotropic. Then the following discussion of the implications of assuming isotropic diffuse light is important (and necessary), but it does not fit in this section of the paper.

Line 71 – This section describing HSR1 is insufficient. Here is where some of the content from the introduction should go – describe the shadow mask, the specifics of the HSR1 design including a description of the spectrometers used, the theory behind the diffuse and total irradiance measurements, what is new about HSR1 specifically compared to past iterations of the instrument, briefly overview the calculation of the direct irradiance, etc.. Second, how is HSR1 calibrated? At the very end of the manuscript in the Discussion section it is noted that the HSR1 is calibrated against a lamp standard -- why is that procedure not described here? Why is there no discussion of the cosine response of the HSR1? Field-of-view issues, and lensing effects of the dome are briefly mentioned but that discussions lacks detail. It is useful to understand the limitations of the HSR1 instrument so the results of this intercomparison can be interpreted.

Line 74 – what is the native sampling rate of HSR1? Or how many data points are getting smoothed over in 1 minute?

Line 78 – roughly how far apart are the different instruments from each other? It is not easy to infer this from the reported coordinates. A map detailing the locations of the various instruments would be useful.

Line 79 – I would move Figure 1 that shows an example of HSR1 irradiance data to a later part of the paper. Also, why not include the comparable irradiance measurements from the other instruments?

Line 90-95 – This discussion of the cause of the downtime seems unnecessary.

Line 98 – How was it determined that stray light is causing the noise at the tails of the spectrum?

Line 102-103 – the comment on future designs of the HSR1 is better suited for a discussion section later in the paper.

Line 110-111 – “We consider how the dome lensing effect corrected total and diffuse spectral irradiances may affect the results in Sect. 5.” I do not believe that this was ever done in Section 5.

Line 115 – throughout the manuscript I am not sure it is necessary to refer to measured or derived quantities at a specific wavelength as “spectral”. As far as my knowledge goes this is not standard practice. It is more readable to just say the AOD at 500 nm, for example.

Line 130 – again, I am not sure the discussion of data reprocessing is necessary.

General comment – there needs to be more discussion of the magnitude and sources of measurement uncertainty for HSR1 and the reference instruments. This will help the reader better understand the significance of the differences between the measurements.

Line 145 – how do visible and sub-visible cirrus impact the determination of clear-sky periods? Cirrus can significantly bias the diffuse irradiance measurement.

Line 149 – what does this manuscript gain by including the comparison of PAR? I recommend removing this portion of the analysis.

Line 170 – it should be made clear that in equation 2 the optical depths have a spectral dependence.

Line 192 – what is the rationale for kicking out half of the data points when deriving the TOA DNI?

Line 227 – “Therefore, portions of the surface downwelling diffuse light are not measured by the HSR1 and...” It seems like this light is measured by the HSR1, but it is just attributed to being direct irradiance?

Line 228 – Throughout the paper comparisons are done between the various reference instruments. As currently written, this seems unnecessary as I do not see what value it adds to the analysis, and it distracts from the main topic which is the evaluation of the HSR1.

Line 251 – In Figure 4 why not also include the direct irradiance?

Line 255 – This table is hard to read and interpret and does not hold a lot of utility to the reader. Much of this information is already stated in the text, so I'd either omit the table or present the data in a graphical format.

Line 271-273 – “Similar to the total spectral irradiance, the MFRSR C1 and MFRSR E13 diffuse spectral irradiance comparison at 940 nm is the largest relative difference, which is nearly an order of magnitude larger than all other wavelengths (0.9-1.9%). This further highlights the challenges in measuring the spectral irradiance at 940 nm.” I found this wording confusing and suggest it be revised.

Line 279 – the details about the MFRSR spectral channel widths seems better suited for Section 2.

Line 287 – what is the motivation for comparing HSR1 to SASHe under clear sky-conditions. As is this manuscript is only presenting statistical quantities of HSR1 versus other instruments with little justification for doing so or interpretation of the results. For example, how might the shadowmask design of HSR1 influence this comparison with a shadowband type instrument?

Line 304-312 – again, what about the instruments or experimental setup is driving these differences.

Line 325 – this sentence should be reworded: AODs are not collected, they are calculated.

Line 340 – again, I do not see the value in the comparison of the AOD derived from the reference instruments.

Line 375 – it may be worthwhile to include a timeseries figure or two of irradiance and AOD that illustrates under what solar zenith angle and cloud conditions there is good and poor agreement between HSR1 and the reference instruments.

Line 423 – “Noting the measurement uncertainty of $\pm 3\%$ in the diffuse flux (Michalsky and Long, 2016), only 16.5% (all times) and 18.3% (clear-sky times) of the diffuse flux errors due to considering the HSR1 diffuse ratio are within measurement uncertainty.” I had a hard time understanding this sentence and would recommend rewording it.

Line 509 – This section is not a discussion section but it is a summary. Here is a good place to discuss how the design of the HSR1 impacts its ability to measure irradiance relative to the reference instruments. Under what conditions does it perform well (e.g., clear-sky, cloudy-sky)? And when there are biases in the data HSR1 produces, why? For example, what impact does the wide field-of-view, the cosine response of the sensor, the assumption that the diffuse light is isotropic, etc., have on the measurements.