Reply to comments

We would like to thank you for reading our manuscript and commenting on it.

The comments are copied and shown below in italic.

Comment.

The manuscript reports some very interesting data and application of various methodologies mainly from the the AERONET/SKYNET calibration framework for two instruments but the arguments for some of the results and conclusions are not convincing or not well explained. As in the part II paper there are a significant amount of assumptions and previous results without references that need to be listed. An expanded and re-written manuscript could fix these issues would be most welcome to all interested in atmosphere-based spectral Sun radiometer calibration.

A brief summary of the major issues is below. Any manuscript for a global audience needs to conform to some international standards of nomenclature. Unfortunately, the authors use 'accuracy' as a quantitative property for the majority of the paper when 'accuracy' is a qualitative term (i.e. good, bad, excellent); only in the last parts of the paper is the term 'uncertainty' used but without any explanation of what coverage factor or degrees of freedom.

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Reply

We rewrote carefully and added explanations.

*Similarly, the paper quite clearly calls aerosol optical depth (AOD) 'aerosol optical thickness'(AOT) when AOT = m*AOD, and it is only through equation (1) at line 327 that what the authors mean by 'optical thickness' becomes clear.*

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Reply

We replaced "optical thickness" with "optical depth".

The paper quite clearly demonstrates the 'calibration constant' of the POM-02 is not a constant for the majority of channels (if any) [and likely true for a number of spectral radiometers!]. Instead it may have been useful to define it as the 'coefficient used on a day that represents the signal at the top of the atmosphere at 1'AU and at a *representative temperature of X degC'. So why persist in using the term 'constant'? This could have been a key conclusion of the paper rather than implied or assumed.*

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Reply

In this paper, calibration constant V0 is the output of the radiometer to the extra-terrestrial solar irradiance at the mean earth-sun distance (1 AU) at reference temperature. However, when comparing V0 by the IML method with V0 based on the Langley method, the temperature correction is not performed.

We conventionally use the word "calibration constant". But, the sensor output depends on the temperature and it varies with the aging of the radiometer. Therefore, V0 varies with time.

The solar constant also changes, but it is called the solar constant.

What is a 'normal Langley method'? There are so many variations of 'the Langley method' in the literature that they could be listed on several pages. No reference was provided for the specific method used, and what was more confusing was the application of at least 4 variants of the 'normal' method resulting in Table 1 - and no reference on how the gaseous applications or temperature correction were done, or the reason for the very high standard deviations in an unknown set of MLO calibrations when gaseous and temperature corrections were applied. The non-description of the applied methodologies and the non-explanation of the variances is an example where some references or further detail is required.

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Reply

We wrote a brief explanation on the Langley method and described the method we did.

The temperature coefficients and their application to the raw signals is a key piece of information for other users. However, the section is another example where minimal methodology is presented. There was no experimental setup provided only that it ' was used to measure the temperature dependence of the pyranometer' or the likely uncertainty of the process and the choice of a representative temperature for each sensor. As written, it could almost be assumed that a single value was applied per 'Langley' period rather than individual measurements, and one would have to guess on the representative temperature. It was also disappointing not to see a comparison of the *derived coefficients to the sensor manufacturers' specification sheets. The description of the temperature environment in the POM-02 is a very, very useful – though one could argue that use of the term 'temperature control' was not appropriate.*

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Reply

We added about the measurement procedure of temperature characteristics.

The temperature correction was performed for each measurement data.

The temperature dependence of the sensor output is approximated by the following equation.

$$V(T)/V(T=Tr)=1.0+C_1(T-Tr)+C_2(T-Tr)^2$$

where $V(T)$ is sensor output at temperature T, $V(T=Tr)$ is sensor output at reference temperature Tr, Tr is reference temperature, coefficients C_1 and C_2 are determined by the least squares method.

Therefore, measured $V(T)$ is corrected by the following equation.

$$V(T=Tr)=V(T)/(1.0+C_1(T-Tr)+C_2(T-Tr)^2)$$

Instruments are designed to operate the heater when the inside temperature is less than 20 or 30 °C. For colder regions such as polar regions, the setting temperature is 20 °C, and in other regions, the setting temperature is 30 °C. When the temperature near the rotating filter wheel inside the instrument is below its threshold temperature (20 or 30 °C), the instrument is heated. When the temperature exceeds the threshold, heating is stopped. However, there is no cooling mechanism for when the temperature inside the instrument is higher than its threshold temperature for optimum operation. The temperature control setting of POM-02 (Calibration Reference) is 20 °C, and that of POM-02 (Tsukuba) is 30 °C.

According to the specification sheet, the temperature dependence of the detector sensitivity is almost zero (cannot be read) at wavelengths from 300 nm to 950 nm. At a wavelength of 1020 nm, it is about 0.2%/deg. At wavelengths of 1225 nm, 1627 nm and 2200 nm, they are almost zero, −0.05, 0.02%/deg, respectively. The temperature dependences of sensor output shown in Figs. 2 and 3 are the characteristic of the entire instrument. Some channels exhibit greater temperature dependence than the temperature dependence of the detector.

In section 4 (line 180+) the results of the ’normal Langley method’ are described in terms of ’errors’ but there was no reference only a mean (weighted by an unknown weight or unweighted) hence use of the term ’error’ is inappropriate for an unknown parameter of a probability distribution. But an examination of the table suggests these just the (unbiased) standard deviations and therefore only contribute to a single component of the total uncertainty of the ’normal Langley method’.
As indicated previously, no indication is given for the increase in this uncertainty component when the sensor signals are corrected (in a manner unknown) when compared to no temperature correction.

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Reply

The term “error” is not an appropriate word. According to comment from other reviewer, it was rewritten as “relative standard deviation or coefficient of variation”. In any case, as we mentioned in the text, it is SD / V0; SD:standard deviation and V0 is mean value.

The lines 232 to 244 describe the likely variation in the ’calibration constant’ obtained at MLO over a period of years for the reference POM-02, and summarized on Figure 5 which has a log scale likely because of the range in the Vo values. If the variation is important, then the results should have been scaled to say the 2010 calibration. It would then also be a better lead into the discussion of the interpolation method (and associated uncertainty) that could be required to ensure a required uncertainty (i.e. 2% for high AOD environments for an unknown air mass range - see the WMO (2005) for the working POM-02.

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Reply

In Fig. 5, we only want to show that V0 changes with time and its rate of change depends on the wavelength. We would like to keep this figure as it is.

The discussion on the reasons for the seasonal variation of the ILM was not convincing, and the lack of opportunity to perform of verification by using calibrating the working instrument with the reference instrument when the seasonal peaks and troughs of the ILM occur was disappointing. Given that the selection of true or apparent solar zenith *angle, the airmass type, the rate of change of airmass, and the airmass range used are known to have a seasonal impact on derived 'calibration constants' derived from almost any Langley method variant it was disappointing they could not be examined even for the 500 nm channel of the Tsukuba POM-02.*

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Reply

Until now, no one has pointed out that the V0 determined by the IML method changes seasonally. First of all, we want POM-02 users to know this fact. As you say, many factors are changing seasonally. It is very difficult to clarify which seasonal changes are related to the seasonal change of V0 determined by IML method.

Let the difference between V0 determined by the IML method and V0 interpolated from V0 determined by inter-comparison with the reference POM-02 be ΔV0.

Since ΔV0 is correlated with the optical thickness and the fitting coefficient of the IML method, we believe that V0 determined by the IML method is related to optical thickness and refractive index.

The Improvement of the IML method is a future task.

The authors applied a variant if the general method for the calibration of near-infrared channels. It is a pity that it wasn't applied to all wavelengths either using the reference POM-02 AOD or the most stable channel of the Tsukuba POM-02, and hence also test the small variations in wavelength over time. The comparison of the general and ratio (i.e Dobson spectrophotometer) method results to the 'AERONET/SKYNET" methodologies that have not changed since the inception of AERONET and largely based on the hand-held sunphotometer comparison procedures developed at NOAA by Ed Flowers in the 1960s would have been very interesting given the breadth of the excellent JMA data set.

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Reply

We applied this method to all channels and obtained similar results. However, we are interested in the 1225, 1627, and 2200 nm channels which are not calibrated by SKYNET. Therefore, we showed results only for them.

The method we tried in this study is not used in SKYNET. We have a plan to use SKYNET data to verify the aerosol optical thickness retrieved from the satellite data. The aerosol optical thickness in the shortwave-infrared region cannot be estimated in

SKYNET. Therefore, we tried this method by ourselves.

The data used here is the data taken by JMA research branch (JMA/MRI). It is not the data taken by JMA routine observation branch. The JMA routine observation branch recently started observing with POM-02, but they do not have the technique to calibrate POM-02 by themselves. They participated in the 4th WMO Filter Radiometer Comparison (Kazadzis et al. 2018). Although we are not in the author of this paper, the calibration constant of POM-02 used by them was transferred from the POM-02 (Calibration Reference) in this study by the method shown in this paper. In this inter-comparison campaign, aerosol optical thicknesses at the wavelength of 500 nm and 875 nm were compared. The results of comparison showed that JMA's POM-02 achieved WMO criterion (WMO, 2005). We believe that the method shown in this study is adequate.

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