We would like to thank Dr. Doppler for his comments and help on improving the manuscript.

SPECIFIC COMMENTS

- Figure 3: Comparing the uncertainty bars and the slopes, this figure does not show an improvement of the quality of the calibration, when WORCC moved the calibration site from Davos to Izaña. To which is IZO a site of better quality for Langley calibration than PMOD-WRC?

Figure 3 is not an attempt to compare the Davos and Izana sites in terms of Langley potential "quality". Figure 11 shows the monthly variability for both sites and it is clear that with the exception of the Izana dust intrusion months, Izana shows much lower AOD variability. Back to Fig.3, bars from the Davos period is not directly linked with Langleys but is a mix of Langley transfers and comparison with existing instruments, while the Izana period is based purely on Langleys at the site.

We have investigated the Langley quality at Mauna Loa and Izana in a forthcoming work. Using 15 years of sun-photometer measurements at both sites we concluded that the effect of the aerosol variability at each of the two sites in the Vo determination uncertainty is 0.3% and 0.5% respectively. These percentages are directly the standard deviation of the Vo distribution of all (15 year's) Langleys for each site.

- Chapter 3.1. – Instrument calibration: In the case of a calibration against the triad. What is the strategy? Do you try to have a representative panel of airmass and AOD or do you prefer to focus on low AOD in order to test the sensitivity for low values (avoid negative values, improve detection of AOD for low aerosol masses)

A text was added to the document:

"In practice, when an instrument is calibrated against the triad, the only limitation on using the synchronous signals is the cloud presence. So no air mass or AOD limits are included."

- About criteria for the statistics:
For monthly statistics: "A minimum of 30 hourly values is required" -> implies 2 days of measurements are enough if they are full and in summer. Is it reasonable ? Don't you want to introduce a criterion of amount of days per month?

Yes there is a criterion for days per month also that was added now in the text:

"A minimum of 30 hourly values and 10 days per month are required to calculate the monthly mean."

However it has to be noted that monthly values are not officially submitted to any database so the limits could depend on the potential application that these monthly means are used.

No criterion of repartition of the minutes during the day. My question about the daily mean: Maybe for one site we have, because of the clouds or the availability of the horizon (mountains), a morning average and in other places an afternoon average - Would the comparison of the daily AOD at these 2 sites be still pertinent?

No there is no criterion of repartition of the minutes during the day.
In cases that there are instrument horizon problems in one site then there can not be any repartition as blocked by mountains measurements will be always out of the L3 data.
In the case of a site with consistent cloudy conditions in specific parts of the day (e.g. morning): We think that since AOD can be measured only in cloudless conditions at the particular site mean AOD have to be calculated only by the non morning parts of the day that are cloudless, even if there is a certain daily pattern for this. That is because, for example if someone would like to study the aerosol radiative forcing for the site he/she can not use the morning (cloudy) measurements anyway, so AOD retrieved from the rest of the day is the representative AOD for the site.

Other Specific comments

- In the introduction (1.21), you cite "AOD has been measured with the use of sun-photometers for more than 50 year (Holben et. al., 1998)". I have two comments to this:

o Holben et al. 2001 (also in references' list) describe better and longer the 50 years long history of AOD measurements with sunphotometers than Holben et al. 1998

I really suggest you to briefly describe this 50 years story of sunphotometers, and more expansively than Holben et al. did. All the authors of this manuscript are staff members of PMOD-WRC, a very historical institution, this is why, the reader expects from you that you have the ambition and motivation to make this historical description by your own. You can cite Volz (1959, 1969), Flowers, Shaw (1976, 1982), Leiterer and Schulz (wmotd 222, 1988) And maybe more recent articles describing long time series at specific sites (Weller and Gericke [Met. Zeit. 2005] for MOL-RAO Lindenberg, Barreto et. al [AMT 2014] for Izana.

A paragraph has been added

Atmospheric extinction of sunlight has been studied at least since 250 year ago (P. Bouger). Linke (1942) turbidity, Angstrom (1929) extinction power law and Junge (1952) with the relationship of particle volume and aerosol number size distribution have mainly set the theoretical basis on studying aerosol extinction. However, Volz (1959) have developed a sun photometer able to measure atmospheric turbidity in different wavelengths using filters, used in the first (U.S.A) (Volz, 1969) and the first European Flowers (1969), Network of turbidity measurements. Since then various sites have included AOD measurements to their monitoring schedule constructing long term series of AOD (e.g. Barreto et al., 2014, Weller and Gericke, 2005, Nyeki et al., 2012). Most of these measurements are site-specific, with little relevance to long term trend analysis on a global scale, however, more recently, several multi-year spatial studies (Holben, 2001; Che et al., 2015, Mitchell et al., 2017) have been conducted.

- In the introduction (2.17), you mention that "GAW-PFR aims to provide inter-comparison information between networks by overlapping sites". -> Is it only an objective (aim) or are there already studies that make inter-comparison of networks? If there are some studies, please mention them and cite the corresponding publications.

Unfortunately there are not too many studies published. There are actually three studies under preparation for AERONET (Izana and Mauna Loa) and SKYNET but they can still not be cited now.

- Chapter 3.2. – Other issues (13.13-21): The QM parameter tested is well described. Could you inform about the threshold of spectral shift that your QC politic allows for the spectral shift of the spectral channel?

In PMOD WRC we actually do not characterize PFR filter information frequently. Measurements on two instruments showed a shift of less than 0.2 nm. Filter specification provide a central wavelength with an accuracy of ±0.7 nm. Such measurements are possible when instrument that are calibrated show Rayleigh scattering related deviations pointing at the direction of filter shifts. Such cases are not observed till now.

- Figure 10, Page 14: It is well shown how each method detects or not some type of clouds. Could you explain what are your own QC using all these different methods? Which data you keep in the Level 2 or Level 3 of GAW-PFR database and which you flag out because you consider them as cloud observations.

A paragraph was added:

"It has to be noted that final AOD data produced include all available measurements that have passed the quality control procedures, except the cloud flagging ones. So all reported AODs are available, accompanied by a flag showing if and which one or which combination of cloud flagging criteria have been assigned for the particular one minute measurement. "

TECHNICAL COMMENTS

3.1. Citations / references An effort has to be done concerning the reference citations:

All references related recommendation have been included in the new manuscript

3.2. Mathematical formulae (equations):
The quality of the formulae has to be improved. If you use a parameter terminology in a formula it has to be defined in the formula block or in the text above or below. Do not hesitate to write more formulae in order to help the reader to follow the mathematical reasoning.

- Equation 1: In the current version of the manuscript, the paragraph introducing equation 1 (2.32 – 2.37) is unclear. I suggest you to cite before Beer Lambert in the

atmosphere (transmission = exp(-(tau_aer + tau_rt)) and to write the equation T = I/I0, then only write the equation (1) as a consequence of the 2 others.

Changed

- Equation 3: please explain each term used in the equation. Is Nref the number of referent instruments (in this case of a triad Nref = 3)? What is the origin of the factor 1.96?

1.96 is the approximate value of the 97.5 percentile point of the normal distribution used in probability and statistics. 95% of the area under a normal curve lies within 1.96 standard deviations of the mean, and due to the central limit theorem, this number is therefore used in the construction of approximate 95% confidence intervals. Its ubiquity is due to the arbitrary but common convention of using confidence intervals with 95% coverage rather than other coverages (such as 90% or 99%). This convention seems particularly common in medical statistics, but is also common in other areas of application, such as earth sciences and social sciences

There was an error in the formula (a ^{2} was missing)

$$U95 = 1.96\sqrt{\sum_{i=1}^{N_{ref}}\left(\frac{\bar{V}_{0xR} - V_{0x}}{2\sqrt{N_{ref}}}\right)^2 + \sum_{i=1}^{N_{ref}}\left(\frac{\sigma_{0xR}}{\sqrt{N_{days}}}\right)^2}$$

Where $\bar{V}_{0xR}$ mean calibration constants derived by the reference instrument R and averaged over all comparison days (Ndays). V0x is the final calibration constant calculated from all comparison days (N_{days}) and all reference instruments (N_{ref}) .

- In the text (9.32) You mention the average values <V_0xR> (in the text with a bar for average). Is it an average over the days? Over the number of measurements?

Text added

The average, over the number of measurements over a day, values $\bar{V}_{0xR}$

3.3. Other technical comments

- (3.4) The origin and computing of U95 is unclear. Can you repeat the GAW/WMO rules in the text and give a citation from a publication or a GAW report explaining U95 in detail?

Text was added

According to WMO, 2005, as traceability is not currently possible based on physical measurement systems, the initial form of traceabilty will be based on difference criteria. That is, at an inter-comparison or co-location, traceability will be established if the difference between one network's AOD and another's is within specific limits. Those limits for finite field of view instruments have bene set (WMO, 2005) to 0.005 + 0.01/m optical depths and the acceptable traceability is when 95% of the absolute AODs are within those limits.So requiring 95% uncertainty (U95) within ±0.005 + 0.01/m optical depths, where the first term (0.005) is linked to instrument uncertainties (signal linearity, sun pointing, temperature effects, processing, etc.) and the second term to a calibration uncertainty of 1%.

- Figure 1: The legend of the left picture is not readable (the points that specify the colors of the wavelengths are to small)

Corrected

- (5.14). When you talk about uncertainties, please precise if you are discussing an absolute or a relative uncertainty. This would help a lot the reader who tries to follow the reasoning

Text added

Based on Eq. 1 the AOD absolute uncertainty, δAOD_{Vo} that is related only to the Langley calibration factor equals $\frac{\delta \ln(Vo)}{m}$ where *$\delta ln(V_o)$* is the uncertainty in *$ln(V_o)$*.

- (6.1-3).

It is hard to understand the relation between delta_AOD_V0 and delata_ln(V0), maybe one equation more would help

An explanation is provided based on the (new) equation 2.

- (10.10) "in addition we have calculated the V0_U95

Done

". But what is shown on the Figure 6 under the denomination "U95(%)"? Is it V0_U95? Is it CV? Is it something else?

Figure axis text was corrected, it is Vo_U95 in %.

- (14.3) "AOD > 2" I guess it is AOD[500 nm]?

Corrected

- (15.4) "AOD(lambda1) > AOD(lambda1)" -> "AOD(lambda1) > AOD(lambda2)"

Corrected