Author's response to the comments of Anonymous Referee #1 and #2.

First of all, I would like to apologize for my delay.

Author's response to the comments of Anonymous Referee #1. - bellow

Author's response to the comments of Anonymous Referee #2. - from page 12

Author's response to the comments of Anonymous Referee #1.

Authors comments are written in bold.

GENERAL COMMENTS

The manuscript by dr. Peter Hrabčák presents a long dataset of total ozone and aerosol optical depth (AOD) measurements taken by a Brewer spectrophotometer at the site of Poprad-Gánovce. The optical depth of these two atmospheric constituents are calculated and compared. A basic statistical analysis of the annual averages is presented, which supports a statistically significant decrease of the AOD. Comparisons with satellite estimates and zenith sky measurements are also briefly reported.

An in-depth analysis of such a long dataset (23 years) is certainly of interest and useful for the atmosphere and climate communities. However, I don't support the publication of the manuscript until major revisions are made, in particular:

1. the language must be definitely improved for ease of reading;

2. the statistical analysis is too simplistic:

- an in-depth examination of the measurement uncertainty is essential to trust the quality of Brewer measurements and to correctly calculate the trend significance, however it is missing in the manuscript. The calculation of the statistical significance was performed on the basis of the natural/instrumental variability from the data themselves. Instead, systematic errors and drift uncertainties should be included in the calculation of the trend uncertainty and its statistical significance. Furthermore, the paper would certainly benefit from comparison to co-located (or close) photometers, if available;

The manuscript was significantly redesigned. Section 3.1 Comparison of AOD values obtained by LPM, BSM and Cimel sunphotometer and section 3.3 Correction for the diffuse radiation, stray-light effect and polarization were added, among other things.

- the observed decrease in the AOD series seems to be mainly introduced by the first two years of data (Fig. 3-4). Since "instability of ETCs during the 1st calibration period" is reported in Sect. 3.1 and the trend abruptly changes

after 1996, I'm wondering if use of a single trend for the full series is justified and what the statistical significance of the trend would be by considering only the 1996-2016 period;

Among other things, this answer is included in the manuscript (page 19 , line 15): It is obvious already at a glance that the AOD, unlike the total ozone optical depth, exhibits an apparent decline for the monitored period. For the wavelength of 306.3 nm, the value of the trend is –0.07 ±0.01 for 10 years, and the value of the trend for the wavelength of 320 nm is –0.06 ±0.01 for 10 years. If we omit first 2 years due to their lower reliability, then the value of the trend for the wavelength of 306.3 nm is –0.05 ±0.01 for 10 years, and the value of the trend for the wavelength of 320 nm is –0.04 ±0.01 for 10 years.

- the results of the analysis are not properly supported by a complete understanding and explanation of the physical phenomena at their base.

The part Methodology and the part Results and discussion was redesigned.

The reported trends are not compared to the existing scientific bibliography.

Manuscript contains this part (page 2, line 11): Anthropogenic emissions of aerosols have been gradually reduced in the developed countries, and a drop in the aerosol optical depth (AOD) has been observed in several locations (Kazadzis et al., 2007; Mishchenko and Geogdzhayev, 2007; Alpert et al., 2012; de Meij et al., 2012; Zerefos et al., 2012). Moreover, introduction was enriched by the examples of trend calculations in the other places.

These remarks are elaborated in the next sections (specific comments and technical corrections).

SPECIFIC COMMENTS

- Sect. 1 (p.3, l.5): the Langley plot method should be explained in Sect. 2.3 instead of the Introduction.

The explanation of the Langley plot method was relocated to Sect. 2.4 instead of the Introduction.

Instead, a list of previous publications about AOD and Brewers and trend calculations would be welcome in Sect. 1;

It was accepted. Please, see section 1.

- Sect. 2.1 should be rewritten in a more rigorous way. Use bibliographic references and/or formulae instead of a qualitative description.

The section has been rewritten, but now the content is in the section 2.2

Moreover, zenith sky measurements should be introduced and explained here and the ZS retrieval algorithm should be presented (how was the ZS polynomial determined?);

The issue of ZS measurements has been removed from the manuscript. It was done because this issue is complicated and the scope of manuscript is moreover quite big.

- Sect. 2.1: possible uncertainties due to the use of a single monochromator should be discussed. The sources of the deviations from the Angostrom law of the AOD measured by single Brewers at the shortest UV wavelengths (e.g., Fig. 5) are known since a long time (cf. Arola and Koskela (2004), "On the sources of bias in aerosol optical depth retrieval in the UV range").

It was accepted. Please, see section 2.2.

Uncertainties arising from polarisation effects and temperature changes inside the instrument should also be mentioned;

It was accepted. Please, see section 2.2.

- Sect. 2.3 and 2.4: some inconsistencies can be found in the use of different cross sections datasets in the manuscript. Effective IUP cross sections are used for the calculation of ozone optical depths, while ozone retrievals are performed using Bass&Paur (cf. Redondas et al., 2014). Bodhaine et al., 1999, is used for AOD calculations, while the Brewer operational Rayleigh cross sections are used to retrieve the ozone optical depth, causing a bias of about 3 DU (cf. Carlund et al., 2017). The authors should explore those issues;

The total column of ozone was recalculated by Bodhaine et al., 1999 and IUP. Now, it is the same like for the optical depth calculations. This issues were explored in section 2.3 and section 2.4.

- Sect. 2.4 (p.6, l.6-19): explain why all those thresholds were chosen. Explain that an iterative approach was used;

Section 2.4 was sophisticated, but it is not possible to explain all things in depth because the manuscript is quite big.

- Sect. 2.4 (p.7, l.5): the figure content should be fully explained in the text;

Explanation of cloud screening is now included in the manuscript (page 10).

- Sect. 2.4 (p.7, l.13-19): with no thresholds for the minimum number of data for a month and a year, the annual average becomes very sensitive to potential gaps in the series and their distribution throughout the year (due to the seasonal cycles of both ozone and AOD). The author should explain how he dealt with gaps in the series.

During operation of Brewer spectrophotometer did not appear big gap in data. Instrument is in standard operating every day, if possible.

The calculation of the linear trend uncertainty as described in the manuscript only takes into account the natural and instrumental variability. However, also instrumental systematic errors (e.g., radiometric calibration drifts) contribute to the uncertainty of the measurements and the trend and should be taken into account to determine the trend significance. An extensive treatment and description of measurement uncertainties is lacking in the current manuscript;

The section 3.3 Correction for the diffuse radiation, stray-light effect and polarization was added.

- Sect. 3.1: according to Fig. 2, the logarithm of ETC jumps by more than 0.2 at several points. The author should prove that a two-year "piecewise" series of calibration constant is suitable for the calculation and explain why a moving average wouldn't be better.

The moving average probably is not a good idea because a variability of ETCs in the intercalibration period is too high due to inaccuracy of ETC determination and the number of determined ETCs is too low. The two-year interval was set also due to a comparisom with Brewer softwere method. This answer is also included in the manuscript (page 15 , line 3): To calculate the ETC characterizing the entire 2-year period, 17 values of individual ETCs were employed with respect to the long-term average. This number is the same in case of all wavelengths. If the conditions were less strict, there would have been more days, for which it was possible to determine the ETC. On the other hand, the spread of determined ETCs would be wider, which would have a negative effect on the required accuracy. Therefore, the chosen criteria represent an optimum compromise.

"Instrument instability", "straylight effects" and "instrumental problems" reported in Sect. 3.1 should be better explained and quantified.

It was partially accepted. Please, see section 3.3.

Furthermore, could you plot the ETCs obtained by transfer from IOS in the same figure and check whether they agree with the values obtained from LPM?

Values of ETCs were compared. Please, see section 3.2.

Also, Langley plots in urban areas are prone to errors: the ETC variability could originate from AOD curvature centred at noon, which cannot be filtered by any quality criteria (e.g., Marenco 2007 and Diémoz et al. 2016). Are in-situ measurements (e.g., PM) available in the investigation area to exclude such an effect?

It is not possible to investigate PM measurements.

- Sect. 3.2: the AOD trend in the first three years of measurement is about -0.1/year (Fig. 3). Does the author have a reasonable explanation for this large decrease?

It is probably link with the decrease in ETC values. Meaby there are also another influences, for example: natural variability or a decrease in combustion of coal and wood in the area of interest.

A reference to Arola and Koskela (2004) should be included to explain the observed AOD dependence on wavelength.

It was accepted. Please, see section 2.2.

The seasonal variability of ozone is a well-known phenomenon, however the general behaviour drawn in Fig. 6 should be better explained in the text and appropriate references provided. Similarly, the AOD seasonal cycle (Fig. 7) should be explained, the physical reasons for the observed two peaks searched for and the general behaviour in Fig. 8 explained.

It was partially accepted. Please, see section 3.4.

The AOD results should be compared to analogous data already published in the scientific literature;

Manuscript contains this part (page 12, line 32): Pribullová (2002) does not mention the unambiguous dependence of AOD on the wavelength as well. It indicates the lowest AOD in case of the lowest wavelength and presents the highest values of AOD for the wavelength of 310 nm.

- Sect. 3.3: instantaneous measurements from the Brewer should be compared to overpassing satellite estimates instead of daily means. Also, why DS and ZS are not directly compared? This would answer the question raised in the Conclusions ("The reason for this is _probably_ the systematic error of the ozone determination using ZS measurements"). The data selection criteria for the OMI-Brewer comparison (223 days) should be better explained (distances in space and time to Brewer measurements). How were the satellite data obtained (e.g., GIOVANNI)?

The issue of ZS measurements and issue of satellite measurements have been removed from the manuscript. It was done because this issue is complicated and the scope of manuscript is moreover quite big.

- Conclusions: the match between OMI and the Brewer is defined "very good", however the linear correlation index is only 0.5. Some bibliographic references should be provided to prove that the agreement between both instruments is satisfactory compared to similar data in the existing scientific literature;

The issue of comparisons with satellite measurements has been removed from the manuscript.

TECHNICAL CORRECTIONS

- p.1, l.17: replace "terrestrial" with "ground-based" throughout the manuscript. The name of the satellite radiometer (OMI) should be specified in the abstract. Some statistical scores should be included in the abstract to quantitatively support the "very good match" claim;

It is not needed, because the satellite part was removed.

- p.1, l.19 "systematically higher values": the value of the bias should be written;

It is not needed, because the ZS was removed.

- p.1, l.26 "Adverse effects have higher doses of UV radiation...": the words order is wrong;

Manuscript was translated by professional interpreter.

- p.2, l.1 "Very necessary" -> "Necessary";

Manuscript was translated by professional interpreter.

- p.2, l.2 "functioning of the human body": be more specific. "The anthropogenic effect": what effect? Please, reformulate the sentence;

Manuscript was translated by professional interpreter.

- p.2, l.5 "about 5%": specify the considered latitude belt. "even lower": why "even"?

It is clear now.

- p.2, l.17: "it affects" -> "they affect". "ones" -> "one". "aircraft flight" -> "emission from aircrafts";

Manuscript was translated by professional interpreter.

- p.2, l.20: "the significant" -> "a significant";

Manuscript was translated by professional interpreter.

- p.2, l.22-32: I would remove this paragraph, which is too didactic and a bit off topic, since bibliographic references were already introduced;

It has been let, but the introduction was enriched by another important things.

- p.2, l.33: "have a significant role to play" -> "play a significant role";

Manuscript was translated by professional interpreter.

- p.3, l.1: it should be better explained that AOD is not the only quantity describing the radiative effects of aerosols;

It has not been accepted.

- p.3, l.2: "AOD obtained results... and satellite measurements" please rephrase the sentence;

Manuscript was translated by professional interpreter.

- p.3, l.4: "Brewer allows" -> "Brewer spectrophotometers allow". "Optical depth" of what?

Manuscript was translated by professional interpreter.

- p.3, l.8 "there is required...": rephrase the whole sentence;

Manuscript was translated by professional interpreter.

- p.3, l.10: "zenith angles" -> "solar zenith angles";

Manuscript was translated by professional interpreter.

- p.3, l.12: it should be explained why lower latitudes are better and what are the "certain limitations in middle and ... higher latitudes";

In this part are the scientific literature references. In the references can be find details.

- p.3, l.17: "previous method" -> "the previous method";

Manuscript was translated by professional interpreter.

- p.3, l.23: "The Brewer ozone spectrophotometer";

Manuscript was translated by professional interpreter.

- p.3, l.25: "different" from what?. "after pass" -> "after passing";

Manuscript was translated by professional interpreter.

- p.3, l.26: notice that the DOAS method refer to continuous spectral measurements nowadays, while the Brewer only measure irradiance at five wavelengths;

The sentence has been removed.

- p.3, l.28: "predetermined wavelengths" -> what wavelengths exactly, and how many?

Section 2.2 was rewritten.

- p.3, l.29: "It is possible to determine the total amount...";

Manuscript was translated by professional interpreter.

- p.3, l.30 "by following a comparative analysis in the mathematical model...": the sentence is totally obscure to the reader that doesn't know how a Brewer works;

The sentence has been removed.

- p.3, l.32 "feasible": explain why it is possible only at those wavelengths;

Now, it is clear from previous sentences.

- p.4, l.1 "0.006+- 0.002 nm": notice that this is the wavelength increment (1 microstep) rather than the accuracy, which depends on temperature changes inside the instrument and frequency of hg tests;

It was accepted. Please, see section 2.2.

- p.4, l.2: "undergoing" -> "has been undergoing";

Manuscript was translated by professional interpreter.

- p.4 l.6: Sect. 2.2 should be the first section, in order to keep the description of the instrument (now 2.1) and of the algorithm (2.3) close to each other;

It was accepted.

- p.4, l.9 "The content of aerosol in the air, whether...": please rephrase this sentence, which is not clear;

Manuscript was translated by professional interpreter.

- p.4, l.11 "In rare cases, it can also be...": rephrase;

Manuscript was translated by professional interpreter.

- p.4, l.14 "relatively windy": relatively in comparison to what?. A figure with a map with the position of the site (e.g., including a wind rose) would be helpful;

It has been rewritten.

- p.4, l.17 "Measured total ozone values... we used...": wrong words order;

Manuscript was translated by professional interpreter.

- p.4, l.19 "density" -> "power density" or "irradiance";

Manuscript was translated by professional interpreter.

- p.5, l.1 "It is used to using so called an effective": rephrase;

Manuscript was translated by professional interpreter.

- p.5, l.10 "Rayleight scattering" -> "Rayleigh scattering". Please, specify here which dataset was used, e.g. Bodhaine 1999;

It is explained in the manuscript (page 9, line 28).

- p.5, l.17-22: the description of the Brewer data reduction is not clear at all to the unaccustomed reader. Please, rewrite this part or replace with bibliographic references; -

It was partially rewrite.

p.5, l.23 "the arithmetic average is then calculated": how is the effective airmass for the average of those 5 measurements calculated? Remember that air mass doen't vary linearly with time;

It was corrected. Now, air mass is calculated separately for all measurements (for every of five).

- p.5, l.25: "zeniths" -> "zenith angles";

Manuscript was translated by professional interpreter.

- p.5, l.29: "angels" -> "angles"; -

Manuscript was translated by professional interpreter.

p.5, l.30: "determine unknown" -> "determine the unknowns";

Manuscript was translated by professional interpreter.

- p.6, l.1-5: rewrite this paragraph in a more ordered way;

It was partially rewrite.

- p.6, l.30: provide a reference for the airmass formula;

It was accepted, please see section 2.4.

- p.7, l.7: "received" -> "ended with";

Manuscript was translated by professional interpreter.

- p.7, l.13 "characteristics ... progressed": the sentence is unclear;

Manuscript was translated by professional interpreter.

- p.7, l.18: "standard deviation" of which quantity?

It is a standard deviation of a given linear trend.

- p.8, l.8 "is the highest for the shortest wavelength": rephrase. "Variation coefficient" -> "The variation coefficient";

Manuscript was translated by professional interpreter.

- p.8, l.11: "A graph also shows ETCs values which characterize entire" -> "The graph also shows the ETCs values which characterize the entire";

Manuscript was translated by professional interpreter.

- p.8, l.12 "25 ETCs were used on average": 25 in a year?.

For entire intercalibration period, it means 2 years.

"Strict conditions met" -> "were met";

Manuscript was translated by professional interpreter.

- p.8, l.15: "the chosen criteria are the optimal compromise". "Directly affects the resulting values";

Manuscript was translated by professional interpreter.

- p.8, l.16: what are the "weather effects"? Does the author refer to the clouds?

Yes, it means mainly clouds.

- p.9, l.14-18: provide bibliographic references of previous studies;

There is no references, it is my own calculations.

- p.9, l.17 and 18: "what" -> "which". Line 21: "than" -> "that". Lines 21-23: several "the" are missing;

Manuscript was translated by professional interpreter.

- p.9, l.24: "the comparison". L.27: "the mentioned". L.28: "in studied" -> "at the studied";

Manuscript was translated by professional interpreter.

- p.10 Fig.3: use the same wavelengths for ozone and AOD optical depths;

I think, it is not a good idea.

- p.11-12, Fig. 6-7: draw AOD and its standard deviation on the same scale, e.g. using boxplots;

I think, it is not a good idea.

- p.11, l.19: "it occurs" -> "occurs". Line 20: "characteristic" -> "characterized";

Manuscript was translated by professional interpreter.

- p.12, l.12: "in particular";

Manuscript was translated by professional interpreter.

- p.13, l.8 "once a day": provide overpass time;

It is not needed, because the satellite part was removed.

- p.13, l.9 "scored specific place": rephrase. Line 10: "the square" -> "a square";

Manuscript was translated by professional interpreter.

- p.13, l.13: "is illustrated comparison of the annual averages..." -> "the comparison of the annual averages of total ozone is illustrated in Fig. 9". "it was the comparison of values obtained ... obtained": rephrase;

Manuscript was translated by professional interpreter.

- p.13, l.20: is 3.9 DU a "significant" difference? Line 21: "It can be said that...": please, explain better which sources of systematic errors have to be considered;

It is not needed, because the ZS part was removed.

- p.13, l.24: what value for the "Angstrom exponent" was used? Why?

The explanation is in the manuscript (page 11, line 5).

- p.13, l.27 "only AOD values smaller than 1.5": how was this threshold established?

The explanation is in the manuscript (page 10, line 9).

- p.13, l.28: I would say that a correlation index of 0.5 is moderate, not strong;

It is not needed, because the satellite part was removed.

- p.14, l.10: "was" -> "were". "so it is a period" -> "i.e. for a period".

Manuscript was translated by professional interpreter.

Author's response to the comments of Anonymous Referee #2.

Authors comments are written in bold.

1) General comments

The present paper includes a long UV AOD series spanning from 1994 to 2016 which may be useful to provide further insight on the role of aerosols on the Earth's climate. Furthermore, these data may be also used to demonstrate the capability of the Brewer spectrophotometer to measure AOD, most of these instruments being used only for ozone measurements.

In my opinion, these two points make the paper interesting for the scientific community. There are however three main issues:

a) There is still room to improve the scientific discussion, see points 2a-i below.

b) It is extremely important to show that the Brewer AOD is correct. For that, you need to provide meaningful comparisons with data from other instruments, see points 2j-k.

c) The quality of the presentation and, specially, of the English has to be improved, see Section 3 below.

Without improvements in these three areas, I can not support the publication of the paper in ACP. With a view to help the author improve the paper, I will provide specific questions and comments next.

2) Scientific discussion

a) On page 3, line 16, the author states that "In this case, the AOD calculation algorithm is part of the main control program for Brewer. The main difference from previous method is that the ETCs for individual wavelengths are not determined by LPM method but they are obtained during calibrating the instrument, i.e. every 2 years". Could the author explain how the ETCs are determined during this calibration of the instrument?

This answer is included in the manuscript (page 7 , line 11): Their size is determined during calibration based on a comparison with the portable reference instrument No. 017.

Have you compared the results from the LPM and the so-called calibration methods?

Yes, I have. Please, see section 3.1 for details.

b) On page 7, Fig. 1, there is an entry with the label "AOD-AAOD<0.5". This part of the screening algorithm does not seem to be explained in the text, and the definition of "AAOD" seems to be missing from the paper.

Explanation of cloud screening is now included in the manuscript (page 10).

c) On page 7, line 13, the author states that "Daily averages are calculated as arithmetic average of all values of a given day (from at least one value)." So, a daily average is considered valid even if there is just one AOD value for the day?

Yes, a daily average is considered as valid.

How many AOD measurements do you obtain on average for each day?

It was obtained 7 AOD measurements for each day on long term average (1994 - 2016).

How many times do you get just one measurement in a day?

It was in 14% of days.

d) From the discussion on page 8 about the calibration periods, it is not clear how they are selected. Are they the same as the period between the standard ozone calibrations?

This answer is included in the manuscript (page 7 , line 14): The LPM applied in this paper employs fixed ETCs for a 2-year intercalibration period, which is identical with the standard intercalibration period for the measurement of ozone. It is assumed that any significant service modifications to the Brewer spectrophotometer during calibration may affect both the calculation of ozone and the calculation of AOD. For that reason, the period not exceeding 2 years was used.

If so, does the author find that the stability of the ozone and AOD configurations are the same?

The base assumption is that the stability of the ozone and AOD configurations are the same.

Did the author try to use shorter calibration periods?

No, I did not use shorter calibration periods.

e) On Page 9, line 14, the author mentions that "ground measurements from the nearby station in Hradec Králové and satellite data" were used to complete the series down to 1962. What type of ground instrument operates at Hradec Králové? What satellite data was used?

This answer is included in the manuscript (page 18 , line 13): It was possible to deduce the state of ozone as early as from 1962 by means of ground measurements from a nearby station in Hradec Králové (Dobson spectrophotometer, 1964–1978) and satellite data (Total Ozone Mapping Spectrometer, 1979–1993).

Could the author show the full series from 1962 to 2016?

It was added graphical illustration (page 18), please see Figure 9: Values of total column ozone amount (TCO) for Poprad-Gánovce, 1962–2016.

f) From Figs. 2 and 4 and text, it's clear that the year 1994 is very noisy for the AOD data. Was this year included in the analyses (specially, the determination of the trends) in Section 3.2?

Yes, this year was included in the analyses. Among other things, this answer is included in the manuscript (page 19 , line 15): It is obvious already at a glance that the AOD, unlike the total ozone optical depth, exhibits an apparent decline for the monitored period. For the wavelength of 306.3 nm, the value of the trend is –0.07 ±0.01 for 10 years, and the value of the trend for the wavelength of 320 nm is –0.06 ±0.01 for 10 years. If we omit first 2 years due to their lower reliability, then the value of the trend for the wavelength of 306.3 nm is –0.05 ±0.01 for 10 years, and the value of the trend for the wavelength of 320 nm is –0.04 ±0.01 for 10 years.

g) Could the author provide some explanation for the behavior shown in Figs. 6 and 7? (E.g., are the peaks be related to weather conditions?)

The behavior shown in figures was commented. For a details please see text near the figures.

h) What conclusions does the author extract from Fig. 8?

The behavior shown in figure was commented. For a details please see text near the figure.

i) On page 13, line 20, the author states that "Such a significant difference is caused by inconsistent methodology for the calculation of total ozone through ZS measurements". Could the author elaborate further why does he consider the ZS measurement method inconsistent?

The issue of ZS measurements has been removed from the manuscript. It was done because this issue is complicated and the scope of manuscript is moreover quite big.

j) On page 13, line 27, while comparing the Brewer and OMI AOD data, the author writes that "The correlation coefficient has reached the value of 0.51 in comparison with each other what represents a strong positive correlation". I don't believe 0.51 should be considered a strong correlation. For comparison, what's the correlation coefficient between the Brewer and OMI ozone data? Instead of the bar plots in Fig. 9, could you plot the fits as in Fig. 10?

The issue of comparisons with satellite measurements has been removed from the manuscript. It was done because this issue is complicated and the scope of manuscript is moreover quite big.

k) As stated above, showing the readers that your Brewer AOD is correct is of the utmost importance. The comparison with the satellite data, as shown in this work and others before, sometimes might not be

straightforward. Making a comparison with other ground-based instruments would be thus a better option. According to the AERONET database, there is level 2.0 Cimel AOD data at 340 nm for the PopradGánovce site from December 2014 to January 2017, see https://aeronet.gsfc.nasa.gov/cgi-bin/type_one_station_opera_v2_new?site=PopradGanovce&nachal=0&year=22&aero_water=0&level=3&if_day=0&if_err=0&place_code=10&year_or_month=1

Could the author use these data to compare with the Brewer AOD? If not, could they provide a comparison with another ground-based instrument? If not, has the author considered the possibility of attending to some inter-comparison campaign?

It was accepted. Please, see section 3.1 for details.

3) Presentation

a) First and foremost, the quality of the English has to be improved. This is not a purely cosmetic question, there are sentences that are very difficult to understand, like e.g. "The lower limit of uncertainty was calculated using average value ETC from which its standard deviation has been deducted in the given calibration period" on page 9. Please, do check the whole paper and improve the English to an acceptable level.

Manuscript was translated by professional interpreter.

b) As mentioned before, it is critical to demonstrate that the Brewer AOD is correct. Such proof should come immediately after the AOD calculation method is presented and before any other discussion of the data. I thus suggest inserting Sec 3.3 (now including a comparison with AERONET data) before Sec 3.1

It was accepted. Please, see section 3.1 for details.