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

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

Author's response to the comments of Anonymous Referee #2. - from page 4

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

Authors comments are written in bold.

The manuscript underwent an impressive revision from both a stylistic and technical perspective.

As already said, the length of the dataset would normally deserve publication. However, I am concerned about the quality of some results, most notably:

1. the spectral dependency of the AOD cannot be reliably assessed, in my opinion, using a singlemonochromator Brewer. For example, the method used by Arola and Koskela (2004) for roughly estimating the effect of the straylight on AOD at the shortest wavelengths, was employed by Peter Hrabčák to correct the measurements, which I think it is not in the intent of the authors of the original paper (the straylight primarily depends on the ozone slant path column, not on the solar zenith angle).

I have followed an instructions in Arola and Koskela, 2004 and Garane et al., 2006. Stray-light effect corrections was calculated according to this inscructions. I did not find in this articles, that the stray-light effect primarily depends on the ozone slant path column and not on the solar zenith angle.

Also, the author does not mention what are the conditions for which he calculated the correction for the light scattered within the instrumental field of view (forward scattering peak should be relevant for larger particles, but how large were the simulated aerosol particles?).

Correction for the diffuse radiation was calculated using the SMARTS 2.9.5 programme (available at: https://www.nrel.gov/rredc/smarts/). The calculations in the SMARTS were implemented for the rural aerosol conditions, which are characterized by the Ångström exponent equal to 0.96 (this information was added to the munascript: page 11, line 22). This Ångström exponent value characterizes the size of the aerosol particles.

I think that the main outcome of the paper (reduction of the AOD and its contribution to the total optical depth) would not be compromised if the AOD for only the 320 nm wavelength, which is the most reliable measurement, were presented;

Thank you for your recommendation. I have decided, that I do not accept it. The main reason is, that it would lead to considerable depletion of the total content of the manuscript.

2. it is not very clear how the uncertainty of the annual averages (and thus, the resulting uncertainty/significance of the trend) is calculated. It is said that the error bars in Fig. 10 are obtained by changing the ETC within the range of its standard deviation.

Yes, it was a way how the uncertainty of the annual averages was calculated. The error bars was obtained by changing the ETC within the range of its standard deviation. The lower limit of uncertainty was calculated by means of an average value of ETC, from which its standard deviation for the given intercalibration period was deducted. The upper limit of uncertainty was determined by analogy. A linear trend was calculated by means of a linear regression using the least squares method. The significance of the trend is defined by the uncertainty of the linear trend. The uncertainty of the linear trend is defined by standard deviation (±σ) of the inclination of obtained line (a little change was occured in the manuscript: page 11, line 14). The values of the uncertainties of the annual averages do not enter the calculation of the uncertainty of the linear trend.

However, how is natural variability taken into account for the assessment of the significance of the trend?

The natural variability was eliminated by annual averages. The annual averages were calculated as an arithmetic average of individual monthly values.

Moreover, using this method, the fewer ETC determinations for each year there are, the narrower is the uncertainty, which is probably not the desired behaviour of the method;

The range of uncertainty interval depends primarily on suitable weather conditions in the given intercalibration period, as well as on the stability and homogeneity of measurements on days, when it was possible to determine the ETC. The number of days, when it was possible to determine the ETC, plays its role too. For instance, there were only two measurements in the first intercalibration period (it covers 1994 and a smaller part of 1995), and for that reason, inter alia, the uncertainty interval for 1994 is narrow and has a very low relevance. In other intercalibration periods, there were at least 8 ETCs. Therefore, the following reliability intervals can be deemed trustworthy (this part is in the manuscript: page 18, line 17).

3. are the results of the standard lamp (SL) test used to recalculate ozone? This could maybe help in further improving the quality of the measurements, especially in periods when the Brewer is unstable.

The results of the standard lamp (SL) test has used to recalculate ozone. The total ozone was calculated using the Brewer Spectrophotometer B Data Files Analysis Program software v. 5.0 by Martin Stanek (http://www.o3soft.eu/o3brewer.html). This software allows SL correction.

From a stylistic point of view, I recommend a revision of the structure of the paper: Sect. 2.4 should be shortened (perhaps moving the most technical parts to the Appendix)

The sect. 2.4 was shortened by removed some equations and its explanations. The reader can find this equations and its explanations in the attached references. The structure of the sect. 2.4 was also revised due to above mentioned changes.

and Sect. 3.3 should be anticipated before the results.

I have also took into account the recommendation of the Anonymous Referee #2: "In short, I suggest to reorder the sections in the following way: 3.3 (corrections), 3.2 (ETCs), 3.1 (validation), 3.4 (long AOD series)". Therefore the sect. 3.3 is now the first part of the Results and discussion. I think that it is better to let sect. 3.3 in the beginning of the Results and discussion, how it is recommended by the Anonymous Referee #2.

A complete list of the technical corrections will be provided once the final publication of the paper

is foreseen.

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

Authors comments are written in bold.

With respect to the previous version, the paper has improved significantly. The author has followed the suggestions of the referees and provided reasonable answers to most of their questions. There are, however, points that still need further attention, as discussed below.

In my opinion, the main outstanding issues of the paper are:

1) On Sec. 3.1, the author presents a comparison between the AOD determined by three methods - his own, the one implemented in the standard Brewer operating software, and a Cimel photometer. This is a great addition which provides the necessary confirmation of the quality of the present data. I have, however, some questions and suggestions with regard to this section:

i) First, I suggest moving this section to just before Sect. 3.4. The current sections 3.2 and 3.3 provide information on the calibration and corrections used to determine the AOD, and section 3.4 provides data retrieved using them, so it should come after them, and just before the whole series is shown. This change would also make easier to understand Sect. 3.1, because the reader would have been introduced to the details of the ETCs determination which are currently provided in Sect. 3.2. Furthermore, I would also suggest changing the order of Sects. 3.2 and 3.3, because it seems that the determination of the ETCs in Sect. 3.2 requires the data corrections explained in Sect. 3.3.

In short, I suggest to reorder the sections in the following way: 3.3 (corrections), 3.2 (ETCs), 3.1 (validation), 3.4 (long AOD series)

It was accepted.

ii) For the 2015-2016 period, the BSM-LPM fit on Fig. 2 doesn't seem to show any offset, the CSP-LMP fit on Fig. 3 shows a small one, and the CSP-BSM fit on Fig. 4 shows a large one. This seems strange at first sight. Can the author provide an explanation?

These average differences are the primary reason for observed offsets on attached charts. As a result of the offset, the intersection of the linear fit is not the same as the intersection of the main axes of the graph. This is the best illustrated by the right graph in Fig. 8, because in this case the average difference has the highest absolute value of all presented comparisons (the same explanation was added to the munascript: page 17, line 15).

What are the values of the intercepts (independent terms) of the fits?

The value of the intercept of the fit is 0.025 for the CSP-LMP comparison (a small offset) and the value of the intercept of the fit is 0.074 for the CSP-BSM comparison (a large offset). The equations of the fits was added to the graphs (Figure 6, 7 and 8). This equations contains the values of the intercepts.

2) The method used to calculate the stray light correction in pages 17-18 does not seem clear to me. In particular, on page 17 the author states that "A ratio of average count rates for four wavelengths in the region from 290 to 291.5 nm to the count rates for the monitored wavelength (one out of five) was determined for 3,386 spectral analyses in total, as well as for various zenith angles of the Sun within them". The author always refer to 306-320 nm as the Brewer operational range, so what are these four wavelengths from 290 to 291.5 nm?

Brewer ozone spectrophotometer (MK IV) performs standard measurements of direct solar radiation (DS) in the UV region at five selected wavelengths, namely 306.3 nm, 310 nm, 313.5 nm, 316.8 nm and 320 nm. It measures also global UV radiation from 290 nm to 325 nm, with a step 0.5 nm (this information was added to the munascript: page 4, line 7).

Why does the calculated ratio provide the stray light correction?

I have added some important notes to the manuscript (for more information please see page 12, line 5). It was not possible to explain all detail because the extent of the munascript is big, but you can find more informations in the article by Arola and Koskela, 2004.

3) Figure 9 on page 18 provides a long TOC series from 1962 to 2016, made up of data from a Dobson spectrophotometer operating from 1964 to 1978 at a nearby site, satellite data from TOMS for the 1979-1993 period, and Brewer data for the years 1994 to 2016. Although I requested this figure in my previous review, now I have some concerns regarding it, in part because of the changes introduced in this revision of the manuscript:

i) If the period of operation of the Dobson is 1964-1978 as written on page 18, where does the data for the years 1962 and 1963 come from?

Excuse me, it was a mistake. Correct period for Dobson is 1962-1978.

ii) Has the author checked that all the three datasets used in the figure are compatible? The Dobson and the Brewer instruments operated at different sites, and ground-based and satellite data can, in my experience, present substantial differences.

You are right, there is also a problem with the compatibility of the three datasets. The compatibility is not clear.

iii) Last but not least, what conclusions should the reader extract from Fig. 9? The focus of this new version of the paper seems to be the determination and analysis of a long Brewer AOD series at Poprad-Gánovce. This figure only seems to allow the author to write that the year 1994 is the minimum of the combined series, but this is doubtful because this year is the least reliable one of the Brewer AOD data, as discussed on page 19. I would thus now suggest removing this figure and related text to keep the focus on the main message of this revision of the paper - the Brewer AOD calibration and resulting data series.

Figure with TOC series from 1962 to 2016 was removed. Also removed was the text related with TOC series from 1962 to 2016. In the text has been made the another changes too (for more information about the changes in the text, please see page 18, line 6).

Besides the main issues mentioned above, there are some smaller ones that should be also addressed:

4) The quality of the English has been greatly improved, but there are still some problems, like e.g. a missing "a" in front of "Cimel" on line 14 of the abstract, a missing "the" in front of "five" in the line 31 of page 5, or a missing "the" in front of "more" on line 7 of page 14, to mention a few. I recommend the author to check again the whole text.

The mentioned mistakes were corrected and the whole text was checked again.

5) In Eq. 2, two different symbols, "m" and the greek letter "\mu", are used to denote the airmasses of different contributions. Although the symbols are explained in the text, it would be easier for the reader to keep just one of them and use subindices to differentiate among the contributions.

It was accepted. For more information please see Eq. 2 (page 7) and related text.

6) The equation of point 8 at the top of page 9 should be explained in words. Also, why was the value 1.75 selected?

Related text was rewrote and enriched about some explanations. Threshold value 1.75 was also explained (for more information please see page 9, line 12).

7) Eqs. 5-7 seem to correspond to the airmasses introduced in Eq. 2. Why "AMF" is used now instead of "\mu"?

AMF in the equations was replaced by the greek letter "\mu".

It would also be better to introduce these definitions just after Eq. 2.

Now the content related to airmass factors follows very close behind the equation 2. But, I have also took into account the recommendation of the Anonymous Referee #1 about the truncation of content of the sect. 2.4. Therefore the sect. 2.4 was shortened by removed this equations and its explanations. The reader can find this equations and its explanations in the attached references. The structure of the sect. 2.4 was also revised due to above mentioned changes.

8) On page 21, line 6, the author states that "the observed characteristics are typical for the central European location of the station". Why? Could the author explain this in more detail and provide some reference to back up this statement?

Related text was rewrote and enriched about a reference (for more information please see page 20, line 18).