

Authors response to comments:

The authors thank Vladimir Savastiouk for the detailed review and comments. See our response and corrections in order to improve the publication:

Review of "TOC intercomparison of Brewer, Dobson and BTS Solar at Hohenpeißenberg and Davos 2019/2020"
Vladimir Savastiouk

This is an important contribution to the continuing efforts for expanding our capabilities in monitoring the ozone layer. The paper describes a TOC intercomparison using the well-established Brewer and Dobson ozone spectrophotometers together with newer BTS array spectrometers. The description of the intercomparison is sufficiently detailed and the conclusion contains important steps for further improvement of the new instruments and the retrieval algorithms. The results of the intercomparison are encouraging.

There are some important shortcomings in the current state of the paper. These are mostly form related, but some are content-related as well.

Comment	Authors response
First, this is likely the longest Introduction I've seen in a such a short paper. I highly recommend cutting it in half. The long list of which reference paper describes which instrument is likely unnecessary.	We think this introduction is helpful since a new type of device is introduced in a long term intercomparisons. This is why literature to established systems might be helpful for some readers. We shortened one section and moved one section into 2.1.
Also to this, an inappropriately detailed description of the Dobson spectrophotometer is out of place in this paper, especially when an exhaustive reference list is provided.	
The authors keep referring to the array-based measurements as "continuous spectral range" and contrast this with the "discrete wavelngths" type of the Brewer and the Dobson. I truly dislike such terminology since the only difference, however important, is in the number of the wavelengths. There is no way to either record or analyze "continuous spectral range". I recommend to either define what you call "continuous spectral range" or not use this term.	We agree that continuous is not the exact expression. We have removed this from the manuscript. We further have clarified and defined the meaning of the expression "full spectrum" in order to distinguish in one expression from the ozone retrieval from the Brewer or Dobson wavelengths.
In lines 142-143 the paper incorrectly states that only one wavelength is used for SO ₂ . In fact 5 wavelengths are used for SO ₂ .	Accepted
Lines 145-150 have a somewhat confusing discussion about the time needed for a measurement in different instruments. The discussion seem to first suggest that both the Brewers and the Dobsons take too long compared to BTS only to finish by saying the indeed it takes up o 5 min for BTS to collect good statistics. I recommend to either express this though clearly as to why you see this important or remove this from the paper.	We modified this section in order to express the capability and our considerations more.
Lines 239-241 must be re-written to a) correctly define what 'm' is and b) to explain how it is possible to have same AMF for ozone, aerosol and Rayleigh (it isn't).	Thank you for this important comment. We agree that the air masses are different for aerosol, Rayleigh and ozone. We have specified in more detail the method used for the retrieval of the presented data. We have clarified this in the revised manuscript. We have specifically written the composition of air mass m in Eq. 2. For ozone, aerosol and Rayleigh, separately (Eq. 2).
Lines 247-248 may need a more accurate statement about why it is possible to retrieve Rayleigh because it is definitely not due to "advantage of the minimal least square fit". Hint: if Rayleigh were to correlate with ozone the retrieval would fail.	We have addressed this comment in the revised manuscript to clarify that only ozone and aerosol are used as fitting parameters of the least square fit. We also highlighted that these parameters are weakly correlated. We agree that correlations would not allow using the minimal least square fit approach. Furthermore, we clarified now that Rayleigh is not retrieved, but used as a parametrization to model the atmosphere.
Line 380 may lead the readers to conclude that the strong seasonal trend is somehow related to the Brewers. Please clarify/re-phrase.	Corrected by removing the relation to the Brewers.

I recommend to re-work the flow of lines 389-395 to have a more logical order of the discussion of the straylight and its effect on the seasonality in the differences.	For better understanding, we have better structured this section in the revised manuscript
Lines 404-405. Assume it's a typo: "too high" meant to be "too low"?	Yes, we corrected this typo.
This is important: almost all figures use a colour scheme that is poor for presentation. Please use more contrasting colours for different lines/points. Also in figures: some lines are only marked as "fit" while no explanation is found how those fit were done.	We agree that figure 6 is not optimal in the color scheme. We adapted this figure. All others seem appropriate. The fits are described in the text or subtitle.
Cosmetic corrections:	
line 12: "fibre-coupled", "optics", "optics"	Accepted
line 21: consider re-wording "the slant path slope" or define what you mean	In the abstract no definition is needed. We added a small definition in line 300.
line 25: "is" instead of "has been"	Accepted
line 107: way too many decimal points for the lat/lon.	Accepted
line 245: re-word "parametrized with a linear parametrization"	Accepted
line 414: "applied" instead of "applicable"	Accepted
line 416: "significantly"	Accepted