

Wavelength calibration of Brewer spectrophotometer using a tunable pulsed laser and implications to the Brewer ozone retrieval

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We thank the referee for their detailed comments on our manuscript. In the attached document we address their questions and suggestions.

Comments by Reviewer #1

J. Gröbner (Referee) julian.groebner@pmodwrc.ch The paper describes a very detailed and unique characterisation experiment of a Brewer spectrophotometer to determine its spectral characteristics (wavelength scale and spectral resolution) which is necessary to calculate the ozone absorption coefficient required for the total column ozone determination from the solar irradiance measurements. The experiment was performed using a tunable laser source to compare and validate the standard procedure used by the Brewer community. The results show that the two procedures provide consistent results to within 0.1% which is very satisfying and confirms that the current standard procedure is valid. Apart from minor grammatical errors the manuscript is well written and certainly interesting to the scientific community. I have a few comments which I would the authors to answer, pending those I support the publication of the manuscript.

page 6, line 10: It is a cubic polynomial fit, not cubic spline.

corrected

- page 7, lines 13-17: It is true that the hg test of the Brewer is repeated when the discrepancy between the actual and determined position is larger than 1.5 steps. However the hg routine sets the position of the micrometer according to the calculation, and repeating the hg routine serves mainly for confirmation. Therefore the hg routine is accurate to ± 0.5 steps, since this is the resolution of the system, not ± 1.5 steps as written in the manuscript. This considerably improves the estimated wavelength uncertainty.

Yes this reduces considerably the error, 1.5 steps are an estimation of the maximum error in the EUBREWNET processing, in this network a mercury test is considered failed when is repeated, the ozone observations are discarded between the last test and the failed one, how ever most of the test gives a correction of less than step.

In section 3.1, I would suggest to add some information on the wavelength uncertainty of the tunable laser setup, which will affect the Brewer wavelength dispersion. I expect in fact the Brewer wavelength dispersion to have less uncertainties when using spectral discharge lamps with published emission line wavelengths ($\approx 1\text{pm}$), than the wavelength obtained by the tunable laser system ($\approx 10\text{ pm}$).

- 5 The text in Section 3.1 was expanded to explain the wavelength calibrations of the instruments used to monitor the laser wavelengths. As to the uncertainty of the Brewer calibration using emission lines, one has to consider also the effect of varying ambient conditions (pressure, temperature, humidity, CO₂ content) on the refractive index of air, which is also in the range of several picometers (e.g. ambient temperature change from 20 C to 25 C has an effect of 2 pm, atmospheric pressure change from 101kPa to 98 kPa would cause a 3pm change). So that the uncertainties in both cases should be indeed quite comparable.

- In section 3.1.2 the authors compare the ozone absorption coefficient calculated with the parametrized and the actual slit functions and show that the difference is of the order of 0.9% (Table 2). The parametrized slits however are trapezoidal, with a plateau at 0.87 (13% from the top). However as shown in Figure 6, this is not representing the true slits, and therefore the parametrization might be closer to reality when using the full triangle as parametrization. This might show that the method using the parametrization with a full triangle will have less differences to the tunable laser results using the actual measured slit functions. (I have made some tests and the full triangle parametrization resolves about half of the 0.9% discrepancy). I would suggest that the authors add a third column in table 2 showing this information.

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Thanks for the suggestion, a new column were added and the calculation repeated, we found the discrepancy is more near to 0.85% +/- 0.1 for the high resolution cross sections rather than 0.9% and the use of the triangular parametrization in fact reduce the difference to the half 0.45%. The results slightly differ from cross section to cross section with the exception of t Bass&Paur probably due his low spectral resolution (Table 1).

Table 1. Ozone absorption coefficient in atm cm^{-1} calculated using four absorption cross sections . And the percentage difference to the measured to the paramerized with a trapezoid and triangular

Trapezoid	Triangular	Measured	% Trapezoid	% Triangular
0.3381	0.3395	0.3406	-0.73	-0.33
0.3331	0.3344	0.3359	-0.85	-0.44
0.3483	0.3498	0.3514	-0.86	-0.45
0.3393	0.3407	0.3422	-0.84	-0.43

- In the conclusion, page 13, last sentence, I do not understand the statement saying that both methods agree to 0.1% if the parametrized or measured slits are used. To my understanding, the standard method using a scanning grating is not able to use the measured slits, since the method relies on interpolating the slit functions to the ozone position, which therefore requires a parametrized slit.

- In my opinion the abstract should also mention the positive result that the tunable laser and the scanning grating method give the same ozone absorption coefficients (to within 0.3% or so)?

The comment were added

1 Minor comments

5 :
 - The different wavelength scales (nanometer, angstroem) used in the manuscript and the figures is confusing, and I would recommend to use a single one (nanometer)?

Following your suggestion, we have modified the Figures and, now, its scale is in nanometers. In the text we have tried to express all the results in nanometers. However, some results were expressed as Angstroem or picomentros, because it is a very small value. (Also, the other referee has indicated that some results may be better expressed in picometers)

-page 4, line 8 : i would explicitly state that the method is an ozone calibration (not to be confused with a radiometric irradiance calibration for example).

revised.

page 5, point 3: The FWHM also depends on wavelength, which therefore requires some sort of parametrization of the slit function when using the standard scanning grating method.

revised.

- Figure 2: The units on the left axis seem too small (maximum of 7 counts/second)?

5

The exponential were missing, now is corrected.

- page 6, line 3: I would remove the value in parenthesis (0.0080 nm), or replace the picometer values.

The value has been removed in the text.

-Table 3, I did not find the acronym for SGW. Could it be added in the caption of the Table, for clarity?

10 The SGW makes reference to the article Weber et al. (2016). In this paper, the authors studied the ozone absorption cross section and its uncertainty.

2 initial review

Page 1, Background: Some references on the brewer spectrophotometer should be included.

References to the brewer instrumet are added.

I have never seen such an equation to determine the ozone absorption coefficient within the beer-Lambert law. I even think that it is a circular argument, because the solar spectrum would be included twice, one time directly in the Beer-Lambert Law, the second time for the convolution. Therefore I think this equation is in fact wrong, and the correct one is the one mentioned later (e.g. equation 8. I would suggest that either the authors can provide a reference for this usage of Equation 7, or delete this part of the discussion, which is not used anyway.

Simmilar equation were used in a tutorial by Davd Wardle during the Brewer workshop,

Wardle, D.: Physical Principles II: Optical Characteristics of the Brewer, in The Tenth Biennial WMO Consultation on Brewer Ozone and UV Spectrophotometer Operation, Calibration and Data Reporting, World Meteorological Organization. [online]

5 Available from: "ftp://ftp.wmo.int/Documents/PublicWeb/arep/gaw/gaw176_10thbrewer.pdf" 2008

and on the Thesis Savastiouk (2005) page 94, eq 4.12

Is a simplified reformulation of the more complex equation of Vernier-Wardle (Vanier and Wardle, 1969) used on the Dobson by Bernhard et al. (2005) (Eq 7)

$$\alpha(\lambda) = \frac{\log \left(\frac{\int E_o(\lambda) S(\lambda, \lambda_i) 10^{-\alpha(\lambda) X \mu - \beta(\lambda) \frac{P}{P_o} \nu} d\lambda}{\int E_o(\lambda) S(\lambda, \lambda_i) 10^{-\beta(\lambda) \frac{P}{P_o} \nu} d\lambda} \right)}{X \mu} \quad (1)$$

10 This references were added to the article:

Page 6 line 10, Figure number is missing

corrected

Page 7, lines 1-8: I think there is a mistake in the discussion in the precision of the hg test: The HG test allows the wavelength setting to be set to within 0.5 micrometer steps, not 1.5. Then, since one step equals (approximately), 0.0075 nm, 0.5 steps would correspond to 0.00375 nm, or 0.0375Å.

Yes there was a mistake, it should be 0.866 steps not Å. The Hg test allows indeed the wavelength setting to be set within 0.5
15 micrometer step. However, the correction is only applied if the difference is greater than 2 steps. According to the Brewer SOP :
“Corrections to the micrometer position are made, and if the adjustment required is greater than 2 steps (.012nm) then the scan is repeated. “ So that we assume that the maximum error is +-1.5 steps with a rectangular probability distribution function. Hence, the standard uncertainty in such a case is 1.5/sqrt(3) = 0.866 steps, which is approx 0.065Å (0.0075 nm per step)

Figure 5. Why use of log scale? In this manuscript, the interest lies in the slit function shape, and therefore in the linear scale, not in the wings.

The reason to use log scale is to visualize the dark count issue, in linear scale is difficult to see.

page 10, last line. I would add "as already noted in Gröbner et al., 1998", at the end of the sentence finishing with (310–320 nm).

5 added.

I am confused by the conclusion, and would suggest that the authors rephrase it be less ambiguous: Point 1 of the conclusion states that the tunable laser results give an ozone absorption coefficient 0.8% higher than the ones obtained with the standard approach using the parametrized slits. The last point states however that the use of the cubic spline agrees with the tunable laser results to within 0.1% and therefore confirms that the use of the tunable laser (fixed grating position), with the standard procedure (rotating grating) is equivalent. While I understand that cubic and quadratic dispersion fittings might give rise to differences, I do not understand how this affects the use of parametrized slits and actually measured ones using a tunable laser.

We rephrase the conclusions, to reflect from one side the use of parametrized slit function gives a significant difference of 0.8% if we use the measured slit, but the use of the laser confirm the operational rotating grating method (using the parametrized slits with the laser)

References

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